SCIENCE

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THE CARNEGIE INSTITUTION.

THE situation that confronts the Carnegie Institution seems to me this: We do not desire a break with the historical development; the energies which have brought about the rise of American scholarship to the present level must work toward its further advance. We are bound to the special conditions and limits of those energies if we do not wish to lose their benefits. Their characteristics, it seems to me, are determined by two factors, utterly unknown, for instance, in Germany. First, the supporting activity in the periphery of the national circle as over against the German governmental support. In Germany the aid came in centrifugal paths, in America in centripetal ones. Secondly, the order of the five hundred higher educational institutions in a sliding scale as over against the sharp demarcation lines of German schools and universities. These two factors belong of course together; both were necessary under the conditions of American history, and their influence must not be impaired, but rather turned to use in planning new progress.

For our concrete question the first factor, the activity in the periphery, seems to me to work in a negative way, as a limitation on all those plans which suggest themselves at the first glance. Everywhere we see departments, laboratories, whole institutions,

in need, scholars complaining that their means for work and research and publication are inadequate, students sighing for fellowships: happy the day when some hundred thousand dollars more every year appear on the horizon. And yet that happiness is a delusion; the loss would be greater than the gain: the help from the inside would stop not the willingness to help, but the willingness to make a strong effort to help on the outside, and just this strong effort in the local groups not only sums up in the long run to a much greater result than any single central aid, but it is also more wholesome, more educative, more adjustable, more American.

I do not speak as a spectator; I feel the want deeply myself. As chairman of the Harvard Philosophical Department I know our desire for the two hundred thousand dollars for Emerson Hall, the corner stone of which we wish to lay next May on the hundredth anniversary of Emerson's birth; we have only sixty thousand at present; how grateful should I be for a Carnegie check of a hundred thousand! As director of the Harvard Psychological Laboratory I should like to have an appropriation of ten thousand every year and should feel sure that, spent for apparatus and equipment for psychological research which I am anxious to make, not a cent of it would go for superfluous luxuries. As editor of the Harvard Psychological Studies I should need just one thousand dollars a year to print the material we gather, of which so much has been lost from lack of funds for publication. I need, thus, the large and the small sums like any one else, and if it be decided that the institution shall take up such patchwork I shall of course look out for my share of the spoil: but if I had to make the decision, I should ignore my small and my large needs and should prefer to go on with my suffering in the higher interest of the scholarly life of the whole country. The institution might build our Emerson Hall, but that would not only take away the pride of all lovers of Emerson in having done their duty, but it would frustrate the hopes of all my colleagues in Harvard who need new buildings or libraries or laboratories or collections for their departments and who would hear in future everywhere the stereotyped answer: why do you not ask the Carnegie Institution? We want to stimulate the trustees, the alumni, the local friends, to build up their institutions by their generosity, by their enthusiasm, by their sacrifice; just such friendly rivalry has built up the intellectual life of the land. Every cent from Washington which disburdens the local officials is an opiate for this feeling of responsibility. It would be the gain of the moment and the ruin for the strongest factor of progress in the long run. The alumni would simply turn to other fields. We know how the aids for the church have slowly decreased and those for scholarship increased; just such a diversion from scholarship would take place, to other good things perhaps, but when we discuss the progress of scholarship we must ignore the fact that other good things stand waiting. And the trustees would imitate the alumni; to-day they scratch their funds together to find some hundreds for a new instrument or for printing expenses; to-morrow when the researchers are considered as provided for from Washington, all ready cash of the college treasurer will flow back again to the undergraduate needs and the higher work will soon be worse off than before.

The only centrifugal help which could be given without harm for the whole system is to be sought in such an arrangement as would aid not special individuals or institutions but the totality of scientific workers. A large printing establishment which un-

dertakes work on a commercial basis but without profit and with a yearly subvention of one or two hundred thousand dollars would bring down the costs of publication to the moderate expenses usual in Germany. If no text-books, but merely monographs, were printed there, an immense gain for the productive scholarship of the whole country might be expected. Large prizes for the solution of certain problems might be another, probably less helpful, scheme which would stimulate all alike without undertaking to support special institutions.

But, as I said, the life in the periphery works essentially as inhibition for central activity; it is the second characteristic feature, the system of smallest differences, the sliding scale of the institutions, which offers positive chances. In Germany where definite types are separated by sharp lines no new development is probable; in America, where the strength has always lain in the possibility of steady progress to higher and higher forms, the same energies must lead beyond the present state. Just as the principle of the sliding scale allowed the graduate school of to-day to grow out of the college of yesterday, we may expect that a higher form, an overgraduate school of tomorrow, will grow from the forms of to-day. And here is the place where the Carnegie Institution might hasten progress. Not a national university which should be in competition with the existing universities, but a higher type standing above all universities, and which, just like the graduate schools twenty years ago, might begin very modestly but might grow in some decades to a great national institution.

The students, or better, fellows, of this school would be young men beyond the doctor examination, young college instructors, men who wish to live some time in the atmosphere of pure research. The teachers would be the masters of the craft, the leading scholars of the country, men of undisputed energy and of original thought. The beginning might be small indeed: all our universities would be greater if half of the professors were left out. In our overuniversity a few great men without any doubtful second class would do, say, fifteen men with a salary of ten thousand dollars each. They would have to come all either for life or for one year; if the one-year system were chosen, they would remain in their own universities and go to Washington on leave of absence. Of course the establishment of such a highest honor in the profession would make necessary some measure of self-government, and herein the institution might become a model for the universities in which the autocratism of the trustees is clearly a relic of the college period but quite unsuited to a university. The German system is much more democratic; scholars choose the scholars, and this autonomic feature belongs to the research-making character of the German institution. The faculty chooses three, of whom the government elects one. In a similar way for instance the physics professors of the thirty largest institutions might propose a candidate for the physics chair, and the trustees of the institution be bound to appoint one of the three who received the three largest votes. There might be fifty fellowships of one thousand dollars to be distributed by the universities.

All this would demand two hundred thousand dollars, and the same sum for laboratory equipments after spending the whole income of the first year for a building. But while of course first-class research laboratories in physics, chemistry, biology, psychology, are essential, I venture even here in the columns of SCIENCE the heresy that the scientific experimental work of such highest institution would not be so important as the synthetic thought which is the one need in our age of scattered specialized

activities. The great problems of principle in all departments of knowledge, not only in natural science, need their temple. The method of the most intimate seminary where in the discussion with mature scholars the thoughts of the leaders develop themselves, would be still more important there than the laboratory method.

The honor of such a supreme court as the highest goal of a scholar would add essentially to the dignity and attractiveness of the academic career. As I said in my 'American Traits,' there are three ways to gain first-class men for productive scholarship, first, advancement in the academic career must be made entirely dependent upon printed achievement; secondly, the beginner must have a chance to remain in the atmosphere of real universities instead of being obliged to go as teacher to inferior colleges, and thirdly, the career must be made more attractive by great social premiums. My words have been sometimes misconstrued to mean only that a scholar would be a better scholar if he had more of the luxuries of life. Even that I believe to be true within certain limits; a larger income would keep more men free from the evil temptations of cheap, paying outside work and other functions ruinous to real scholarly production. But a second-class scholar would not produce first-class work with a steel trust salary. The chief point is not that the men who are inside the fence shall have a better time, but that better men shall go inside because they see what good times, what honors and premiums and laurels await them. And as in every profession the young men are always attracted by the few great premiums at the top, such an overuniversity might do much to gain the first-class men who to-day prefer too often law and business.

That is the point; we have not enough fine men at work and it is not true that the trouble lies in our not discovering them. More than in any other country it is easy in America to discover the first-class man in scholarship if he is really on the ground and has not preferred to go over into banking or law or industry. The American university gives to every man the fullest chance, much more than in Germany, to show his powers, and yet the men are not found. I know one department in which last year three of our leading universities wanted to fill full professorships with first-class young men; two of the places are still unfilled because in spite of the most careful search the men could not be found; and from the most different departments in the country I get every year inquiries concerning German scholars, on account of the lack of really productive men in those special fields here. There is no need of new schemes to discover the extraordinary man; there is need only of schemes to keep him, in the midst of American surroundings, in the field of scholarship, and a new crown at the top of all our universities would be the strongest power. Scholarship would get a new standing in the land and it would be the logical development of the characteristic American factors. American scholarship has suffered enough from the necessary defects of its system; let us not ruin the strength of the system by patchwork interpositions which paralyze the peripheral energies and let us not have unused the tremendous powers of our system, which, through its principle of the sliding scale, allows at every point reached a noble development to a higher creation.

Hugo Münsterberg.
Harvard University.

After the first feeling of happiness over the foundation of the Carnegie Institution, probably every one really interested was forced, consciously or unconsciously, to ask himself in what way this splendid en-

dowment could best accomplish its purpose.

In order to answer the query it seems necessary to take account of the present condition of education and research in our country. If it is taken for granted that the average intelligence and education of our people are fairly satisfactory, and the opportunities for advanced education abundant, there must be some good reason for the paucity of the researches of the highest order. That the country is inimical to the development of high intelligence is negatived in the fields of statesmanship, invention and industry. The reason for the fewness of researches of high order I think is to be found in our governmental and educational institutions themselves. If one looks at the ideal of education in America, it will be found to be that the greatest number shall receive its benefits; and its success, from the smallest country school-house to the foremost university, is measured by the numbers which flock to receive its elementary and advanced instruction. Naturally and inevitably the teacher is overwhelmed by the administrative and teaching duties going with the large numbers of students and the small instructing staff. The few hours he can command on Sundays and in vacations must be used for renewing his strength to meet the every-day routine, or if he is blessed with abounding health and energy he feels that it is only the small investigations that can be undertaken with a hope of bringing them to a conclusion. The greater problems which are ever with him must wait till a hoped-for day when sufficient time, means and facilities are at his command; and these in most cases never come. In institutions not primarily educational the conditions are almost as discouraging. Holders of such positions must also attend to administrative work, and must produce reports to show a reason for being, and immediate results are demanded.

Here then, as pointed out by the founder, is the field for the Carnegie Institution. By supplementing existing institutions and making it possible to free from excessive routine a few who know what ought to be done, and who know how to undertake and carry on researches of the high character contemplated, and who can begin and continue with enthusiasm researches that will require five years to a lifetime even, before results can hope to be obtained. And some also should receive encouragement who will undertake investigations where apparently only negative results will be gained; for often these results are negative only in appearance, and furnish the data by which the most positive results may not be missed.

I am aware that the feeling is quite strong that a considerable share of the income could be most advantageously used in establishing additional fellowships at existing institutions. This would not be wise, for the work done by the holders of such fellowships is, with rare exceptions, more in the nature of education than of research. The young people who fill these fellowships are just learning how to do advanced work, and are by no means prepared to undertake researches of a high character independently. The process of selection has not gone far enough to separate the able student from the one with a genius for research.

While additional fellowships are not advocated, the desirability of finding the exceptional young persons who shall ultimately become capable of assuming 'leadership in discovery' has not been lost sight of. It is believed that the plan proposed of dealing generously with those who have already proved themselves, would most safely and certainly accomplish the desired end. Naturally the older investiga-

tor with a specific problem to solve would select more rigidly than any committee of a faculty, where it is understood that the selection really means the giving of an opportunity to gain a little more education, get a taste of research perhaps, and prolong for a year or two the pleasure of collegiate life. If selected by the investigator for a definite piece of work in the furtherance of the large research, the young man would have opportunity to continue long enough at the work to find out whether or not he could become one of the leaders; and under the wise conservatism of the older man he would gain a safe but powerful momentum which could never be lost.

If, then, it is granted, following the conception of the founder, that the first aim of this institution is to foster research, the practical question for the administrators of the trust is to determine in what way this can be most satisfactorily accomplished.

From my own experience and observation, I think that one of the most important aids it can render is to make it possible for the scientific journals and proceedings of societies in our country, to publish in proper form and with adequate illustrations the scientific results which are actually being produced each year. If one compares the beautiful illustrations in foreign periodicals with most of those in our own, the contrast is certainly painful. How frequently does one see in a scientific journal, or hear through the editor, that illustrations costing over a given—usually very small—sum must be paid for by the author. That is, the author must in the beginning meet most of the cost of his research, and then pay for its adequate publication.

In the second place, as the fund is to supplement existing institutions, the persons selected to carry on researches would naturally work at those institutions where the main part of the plant needed for the investigation is already available. When the person is once selected for a given research, he should be granted absolute liberty of procedure, be given abundant time and generous financial aid.

The specific problem in biology, giving promise of the largest results, it seems to me, is the working out to completion of the entire life cycle of a few forms, rather than the investigation of a detail of structure or of physiology in a great many forms.

It is believed that the thorough investigation of the structure and physiology of a few forms from the ovum to birth, from birth to maturity, and from maturity to old age and death, would most rapidly advance biologic knowledge, and furnish the basis for truly safe and great generalizations. As such research should form part of a solid and enduring structure, it would be of great advantage if the investigator would preserve a complete series—embryologic, histologic and anatomic—of the form whose life cycle was the subject of the research. This series should be deposited in some institution—naturally the one where the work was done—and be open for inspection by competent observers. Such a series would serve not only as a voucher for the validity of the published results, but also to correct errors of interpretation made evident by increased knowledge; and finally it would serve as a basis for further researches.

To briefly summarize: It seems to the writer that (1) The Carnegie Institution is not needed for educational purposes.

(2) Its true place is expressed in the first aim given by the founder—'to promote original research.'

(3) It can most effectively promote research by utilizing as far as possible the facilities of existing institutions.

(4) Its support of the men selected to undertake researches should be generous, and abundant time should be allowed.

(5) The researches most demanded in biology at the present time are complete investigations of the embryology, structure and function of a few forms from the ovum to old age and death.

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THE fundamental principles which ought to control the Carnegie Institution can hardly be better stated than Dr. Cattell puts the matter in the last paragraph but one of his article. It should cooperate with, not interfere with, men and institutions already engaged in scientific work. And I take it that such is the intention both of Mr. Carnegie and of the trustees. The practical question of how this can be done is doubtless the chief problem that confronts the trustees at the outset.

It is my opinion that a *modus operandi* will be found in judicious attempts to meet specific cases. Good as Dr. Cattell's presentation of the whole matter is, nothing he says would appeal to me so forcibly, if I were a trustee, as what he says about work that could be advantageously undertaken in psychology and in the support of scientific publications. For the same reason I should expect from the active workers in every branch of science in the country suggestions regarding their own work and how it could be most effectively advanced. It goes without saying that many requests for help must be denied by the trustees. The perpetual-motion man is bound to turn up, and he will have to be turned down. But between the perpetual-motion enthusiast and the scientific man of established reputation there are many little known but competent workers whose requests for help should receive careful consideration *by committees of specialists.*

Cooperation will mean different things, according to circumstances. Dr. Cattell's scheme of having the Institution appropriate five or ten thousand dollars for this and that bit of work, on condition that an equal sum be raised, reminds one of the estate that *might* have been bought with a pair of boots—if only the boots had been forthcoming. I want very much to undertake a certain piece of geologic work that would require about $10,000, but if the Carnegie Institution offered to give $5,000 for the work on condition that I raised the other $5,000, I should get no help. And there are very few men engaged in university work who could meet such conditions; as a rule university professors are but little in touch with the business world that furnishes the money for investigations of this kind.

Dr. Cattell's suggestion regarding the teacher's sabbatical year seems worthy of attention. The sabbatical year is a great blessing to education and to science, but in many cases with which I am acquainted professors are unable to avail themselves of their leave of absence, because half of their salaries will not support their families and allow them to utilize their vacations in scientific work either at home or abroad. If the Institution could utilize these sabbatical years and pay the men enough to make up the deficiency in their salaries, it would effectively utilize this class of men and would at the same time carry out Dr. Cattell's second principle by improving the condition of men of science.

My suggestions would, therefore, be as follows:

I. The Institution should try to help wherever help is needed and can be advantageously used.

II. It should refrain from unnecessary or unwelcome interference in work already being done by individuals and by other institutions.

III. Care should be taken to encourage scientific work all over the country.

IV. Applications for aid should be received from men engaged in scientific work, and these applications should be referred to committees of specialists for advice.

V. The national government should coöperate with the Institution by providing the necessary buildings at Washington and by permitting, so far as convenient and under proper restrictions, the utilization of the scientific bureaus of the various departments.

VI. Some means should be sought to utilize the sabbatical years of university professors engaged in scientific work.

I have no doubt, however, but that the Institution contemplates doing all these things and many more.

J. C. BRANNER.

STANFORD UNIVERSITY, CALIF.,
September 15, 1902.

I HAVE read with great interest the proof-sheets of the article on the Carnegie Institution. I approve of most of the suggestions. The directors of the Institution must feel their way for a time, and relative values will be made clearer by experience.

The vital principles should be, as stated in the article:

(*a*) To work in harmony with existing institutions, not conflicting with them, and not relieving them from any present necessity of effort.

(*b*) To make the work of scientific investigators freer and more effective.

I should place first in present importance among the many possible lines of work that of helping men who have important investigations or important compilations (as bibliographies) well begun to carry their efforts to a successful end. There are many cases of this kind, in which the worker needs not salary, but help, books, materials, and more often clerk-hire, artist-hire or means of final publication in proper form. Here the exceptional man is already at hand. He will do his work whether encouraged or not. He will bring it to a successful end, if he can have time, help and opportunity.

The establishment of laboratories at Washington for special investigations not yet well provided for seems to me most legitimate. One example of such an institution would be a breeding house or vivarium for the study of heredity and variation on a large scale and with a competent force for observation and record. Such an establishment should be in charge of the man who, whatever his nativity, could make the most out of it. In every branch of knowledge there is some real demand for help of this kind.

I trust that no part of the fund will be used to pay the expenses of students as such, as distinguished from tried investigators. The university fellowship, a fund for paying the board bills of those who may turn out to be scholars, is not gaining in esteem. Doubtless a Carnegie fellowship to one doctor in philosophy out of every ten would help scientific research somewhat, and it could be used—as few fellowships are now used—without danger of pauperizing embryo investigators. But so long as so many better uses for money exist, this one need not be considered.

I do not underrate the value of opportunity to the eager but impecunious student. The free use of a room in the old Smithsonian Tower was once the most valued 'fellowship' open to young naturalists in America. The present writer, among others, feels the sincerest gratitude for the

hospitable 'fellowship' thus extended to him at Washington by Professor Henry and Professor Baird. But its value lay in the acquaintance with scientific men and in the free access to specimens. The reduction of Washington board bills was a mere incident. One duty of the Carnegie Institution should be to make the scientific resources of the Capital available to those who can use them.

In this connection the word scientific should have the broadest definition. It should include historical, economic, literary and linguistic research, all that has a foundation in exact methods.

DAVID STARR JORDAN.

THE IMPENDING CRISIS IN THE HISTORY OF THE MARINE BIOLOGICAL LABORATORY.

THE action of the corporation of the Marine Biological Laboratory, at its recent meeting, August 12, leaves the fate of the laboratory to be decided by the trustees of the Carnegie Institution. It was not a welcome step to surrender the laboratory, but the financial situation seemed to offer no other solution. Some felt very strongly that further deliberation was much needed, but there was danger that delay would prejudice our case with the Carnegie trustees. Compulsory as were the circumstances, it is certain that the corporation and the trustees would have said no to the proposition to surrender, had they felt that our work and plans for the development of a biological center of a national character would thereby be hampered or curtailed. As the matter now stands, it only remains for the trustees of the Carnegie Institution to decide whether they will consummate the steps already taken towards acquiring the laboratory and making it a 'department' of the institution.

In spite of the assurances to the contrary which we have received through one or two of our trustees, I think we may already see that the organization of the Carnegie Institution will necessarily limit our freedom of action and perhaps deprive us of the most essential thing in our independence, namely, *the power to decide upon the nature and scope of our work.* Had such a danger been seen even as a possibility, it is doubtful if the corporation could have been persuaded to transfer the ownership of the laboratory; and had it been seen as a probability, it is certain, I believe, that the vote to transfer would never have passed.

The vote was essentially a vote of confidence in our hoped-for supporters. Only our part of the situation was entirely definite. What the Carnegie Institution would develop as an organization was too largely a matter of conjecture to permit of clear vision. Some points had come out in personal conferences with members of the Carnegie executive committee, but these had not been definitely enough formulated to bring before the corporation. The visible portion of the situation was a debt of about $10,000, doubled by the purchase of land just completed, and an offer of money-relief, contingent on a complete surrender of property rights. It was known, of course, that the transfer of property would make the laboratory a 'department' or 'branch' of the Carnegie Institution, centered at Washington. It was not realized that becoming a 'department' might in some fundamental respects endanger our control of the future development of the laboratory. In fact, we were told by some of those who had formulated the scheme of amalgamation that we should lose nothing essential to our independence, while we should gain a permanent support that was 'almost beyond the dream of avarice'! We were told that if we delayed decision, it would look like lack of confidence, and that we might thus lose

not only the support but also the good-will of the Carnegie trustees. Unripe as the situation was, and unprepared as the corporation was for the final action, circumstances were so compelling that we said no to our doubts and preferences and yes to the Carnegie offer.

Although we have neither asked nor received any guarantees that our freedom of development shall remain unimpaired, it is nevertheless certain that our 'yes' implied trust in the fulfillment of this condition. Few of us, perhaps, had reflected upon the situation sufficiently to realize that barriers might intervene between trust and fulfilment which could not have been anticipated on either side at first. An organization once inaugurated on a permanent endowment is a thing of power. It holds even its authors to a logical development. It becomes law to them and to all who have accepted its authority. The organization of the Carnegie Institution is still *in ovo* in many respects, but as it gradually unfolds it will create classifications and standards to which departments and future developments will have to conform. It is conceivable, even certain I think, that the nature and scope of our plans for development have not been fully grasped by the Carnegie trustees. Can we expect them to shape their organization in such a way as to leave us masters of our own development? If they do not do this, what becomes of the 'trust' and the 'fulfilment'?

We may have the fullest trust in the men behind an organization, and the deepest distrust of the influence which the organization will have on the development of our plans. The organization which they create will define their policy and attitude towards all departments. It will control them and us, and decide for us all what departments shall receive support, and where they shall be located. It requires no prophetic vision to predict that the part will not assimilate the whole.

Hitherto we have been independent. That means that we have been a whole, with the center of interest and the center of authority at Wood's Holl. No one could dispute with us our right to say what departments of biology should be represented here. We could follow our own ideals to the extent of ability and means. All directions of development were open to us. All avenues of support were ours to cultivate and make tributary to an unfettered enterprise. It was on this independence as a foundation that our interests in the present and faith in the future rested. It was the same foundation that sustained the cooperative spirit and the national character of the laboratory. It was our ground of appeal in all emergencies, and the basis of every claim to a wide financial support, the first realizations of which were already at our doors.

The proposition to merge the laboratory in another institution after a fifteen years' struggle for independent existence, at a moment when a strong financial support was on the point of realization, could hardly be expected to satisfy those who had led the struggle, or those who had given the cause unrequited aid and never-failing sympathy. I venture to say that the personal sacrifices already made in the development of the laboratory, the work it has done in research and instruction, the example it has given of the efficacy of cooperation in science, the ideals it has upheld, the national character of its organization, the promising increase of its financial support, all entitle it to hold its independence above any price.

Our attitude towards the proposition has been determined mainly by the desire to secure an immediate and permanent support. While we all agree in the desire, we certainly do not all agree that we can sur-

render the independence of the laboratory with either honor or safety.

It is an undeniable fact that we should all much prefer to have the needed support come to the laboratory rather than see the laboratory go to the support. Why should the support, if it be deserved, not be given to the laboratory, rather than the laboratory to it? Would not the first alternative accord with the declared policy of the Carnegie Institution better than the second? and would it not also better accord with the judgment and expectations of men of science?

It is due to the trustees of the Carnegie Institution to say that the proposition to acquire the laboratory as a condition to supporting it did not originate with them. This is the humiliating side of the situation in which we now find ourselves. They were told that the laboratory was in dire financial distress, that some local western institution was machinating to get possession; in short, that there was an emergency requiring immediate action to save the institution. They were asked on what terms they would consent to own and support it.

When at the conference with the Carnegie Committee the question was asked if they would be willing to support the laboratory without owning it, the reply was that they should have *preferred to give support without taking the whole responsibility of ownership*. It was the 'emergency' that induced them to make the offer of support contingent on our surrender of the ownership to them. It was made clear to us, however, that support without ownership might be considerably less than support with ownership, and that it would have to take the form of a grant to run for a limited time, which might or might not be renewed.

The practical question for us then is: Is our independence plus the possible support by grant from the Carnegie Institution plus the possible outside support, of greater moment to us than a permanent support minus independence and minus outside support? The four elements when taken in the combinations given should be ranked, I believe, in the following order: (1) Independence, (2) outside support, (3) grants, (4) contingent permanent support. Holding independence first, contingent permanent support, which excludes it, must be placed at the foot of the list, as the last resort. The other two elements stand for unknown sums that may be realized on the basis of independence. Outside support, including (1) a definite annual gift pledged for a series of years, (2) cooperative subscriptions from universities, colleges and societies, and (3) individual donations, may be estimated at from $10,000 to $15,000 a year, with prospect of indefinite increase from year to year.

In deciding a question that involves the whole future of the laboratory, it is but the part of wisdom to take a long look ahead. A source of unlimited support, that has an ever-improving prospect for increase, must count for more in the long run than the largest sum to be expected from the Carnegie Institution. Starting with $10,000, which was the annual donation pledged for five years at the beginning of this year, it is next to certain that this sum could have been increased to $20,000 within three to five years. That sum once reached, would not be henceforth a non-growing quantity, shutting out possibilities of endowment and further donations, but rather one with ever-improving chances of enlargement.

This unlimited prospective growth of our present support is as much a certainty as that we shall deserve it. With this growth the cooperative policy hitherto cultivated will remain the best guarantee of the national character of the laboratory. We cannot afford to relinquish the possibilities before us for the sake of an immediate relief which is far from being equal to a per-

manent laboratory, and which, if accepted with all the conditions implied, will prove only a temporary relief, barring the way for greater assistance.

What is $20,000 a year for an all-the-year station, when we are now spending at least $13,000 for a summer's work? If Dr. Dohrn requires not less than $40,000 to $50,000 to meet the annual expenses of the Naples Station, with an average of not over twenty-five investigators, the same plant here would cost about double that sum. At Naples they can charge $500 a year for a single investigator's table. Here there are too many free laboratories to admit of any price on our tables. Moreover, we have to provide for three times as many investigators as they have at Naples, at least for the summer months.

The glowing anticipations of a permanent laboratory rivaling anything in the world, with which we have been regaled, rise far above the $20,000 a year. For the present, at any rate, they are but *châteaux en Espagne*, calculated only to console a premature optimism, which can forsake the larger weal in the distance for the nearer allurement that fetters and mortgages the whole future.

Much as we need now, we have larger needs ahead, for which all avenues of support should be kept permanently open. The support that is given to support, that has the potentials of unlimited growth, that asks not to possess, but only to promote, is something incomparably more precious than any support to which is prefixed the *sine qua non* of absolute possession and authority. It is more precious, not only for all the qualities of disinterested beneficence, but also for the reason that it is essentially cooperative in character, and is thus in harmony with the policy of the laboratory.

Cooperation has been the law and the gospel of our whole scheme of organization. It is the one thing that has given the laboratory unique distinction among marine stations. The prime condition of honest and effective cooperation is an independent organization, representing fairly all interests concerned. Independence has therefore been no meaningless word with us, and hitherto no embarrassments of poverty have tempted us to purchase relief through annexation to another institution.

It is difficult to see how independence can be exchanged for money and cooperation still remain unimpaired. Cooperative support and independence will certainly go, as they have come, together. Can we lose cooperative support and yet keep the cooperative spirit? We can hardly expect to perform the miracle of separating body and spirit.

Cooperative support is a means to an end. It presupposes need, and its realization is possible only under inviting conditions and persistent cultivation. The need alone cannot call it into activity; independence alone cannot bring it forth; and cultivation has no point without the need, and no hope of success under conditions that abridge either the motives or the purposes in view.

The 'atmosphere' or 'spirit' that prevails in the laboratory emanates chiefly from the interaction of sympathies enlisted in a common cause. Cause, responsibility, free initiative, free development, untrammeled policy, all go with independence. The surrender of the ownership of the laboratory reduces it at once to the level of an annex, subordinates its individuality, strips it of final authority, robs it of power to control its own destiny, and subjects its present owners permanently to the condition of petitioners.

If the situation has been fairly stated in its essentials; if the history of the laboratory points the way to its future welfare; if support is deserved at no sacrifice of

independence; if to aid without taking possession would accord with the policy of the Carnegie Institution as well as with the preference of the laboratory people; if this would better meet the expectations of men of science generally, then the trust we have placed in the Carnegie trustees will surely find its best justification in the suggested modification of their proposition to us.

C. O. WHITMAN.

ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

I.

THE members of an association whose studies involve perpetual contemplation of settled law and ordered evolution, whose objects are to seek patiently for the truth of things and to extend the dominion of man over the forces of nature, are even more deeply pledged than other men to loyalty to the Crown and the Constitution which procure for them the essential conditions of calm security and social stability. I am confident that I express the sentiments of all now before me when I say that to our loyal respect for his high office we add a warmer feeling of loyalty and attachment to the person of our Gracious Sovereign. It is the peculiar felicity of the British Association that, since its foundation seventy-one years ago, it has always been easy and natural to cherish both these sentiments, which indeed can never be dissociated without peril. At this, our second meeting held under the present reign, these sentiments are realized all the more vividly, because, in common with the whole empire, we have recently passed through a period of acute apprehension, followed by the uplifting of a national deliverance. The splendid and imposing coronation ceremony which took place just a month ago was rendered doubly impressive both for the King and his people by the universal consciousness that it was also a service of thanksgiving for escape from imminent peril. In offering to His Majesty our most hearty congratulations upon his singularly rapid recovery from a dangerous illness, we rejoice to think that the nation has received gratifying evidence of the vigor of his constitution, and may, with confidence more assured than before, pray that he may have length of happy and prosperous days. No one in his wide dominions is more competent than the King to realize how much he owes, not only to the skill of his surgeons, but also to the equipment which has been placed in their hands as the combined result of scientific investigation in many and diverse directions. He has already displayed a profound and sagacious interest in the discovery of methods for dealing with some of the most intractable maladies that still baffle scientific penetration; nor can we doubt that this interest extends to other forms of scientific investigation, more directly connected with the amelioration of the lot of the healthy than with the relief of the sick. Heredity imposes obligations and also confers aptitude for their discharge. If His Majesty's royal mother throughout her long and beneficent reign set him a splendid example of devotion to the burdensome labors of State which must necessarily absorb the chief part of his energies, his father no less clearly indicated the great part he may play in the encouragement of science. Intelligent appreciation of scientific work and needs is not less but more necessary in the highest quarters to-day than it was forty-three years ago, when His Royal Highness the Prince Consort brought the matter before this Association in the following memorable passage in his Presidential Address:

* Given on September 10, at the Belfast meeting.

"We may be justified, however, in hoping that by the gradual diffusion of science and its increasing recognition as a principal part of our national education, the public in general, no less than the legislature and the State, will more and more recognize the claims of science to their attention; so that it may no longer require the begging box, but speak to the State like a favored child to its parent, sure of his paternal solicitude for its welfare; that the State will recognize in science one of its elements of strength and prosperity, to protect which the clearest dictates of self-interest demand." Had this advice been seriously taken to heart and acted upon by the rulers of the nation at the time, what splendid results would have accrued to this country! We should not now be painfully groping in the dark after a system of national education. We should not be wasting money, and time more valuable than money, in building imitations of foreign educational superstructures before having put in solid foundations. We should not be hurriedly and distractedly casting about for a system of tactics after confrontation with the disciplined and co-ordinated forces of industry and science led and directed by the rulers of powerful States. Forty-three years ago we should have started fair had the Prince Consort's views prevailed. As it is, we have lost ground which it will tax even this nation's splendid reserves of individual initiative to recover. Although in this country the King rules, but does not govern, the Constitution and the structure of English society assure to him a very potent and far-reaching influence upon those who do govern. It is hardly possible to overrate the benefits that may accrue from his intelligent and continuous interest in the great problem of transforming his people into a scientifically educated nation. From this point of view we may congratulate ourselves that the heir to the Crown, following his family traditions, has already deduced from his own observations in different parts of the empire some very sound and valuable conclusions as to the national needs at the present day.

GRIFFITH—GILBERT—CORNU.

The saddest yet the most sacred duty falling to us on such an occasion as the present is to pay our tribute to the memory of old comrades and fellow-workers whom we shall meet no more. We miss to-day a figure that has been familiar, conspicuous, and always congenial at the meetings of the British Association during the last forty years. Throughout the greater part of that period Mr. George Griffith discharged the onerous and often delicate duties of the assistant general secretary, not only with conscientious thoroughness and great ability, but also with urbanity, tact and courtesy that endeared him to all. His years sat lightly upon him, and his undiminished alertness and vigor caused his sudden death to come upon us all with a shock of surprise as well as of pain and grief. The British Association owes him a debt of gratitude which must be so fully realized by every regular attender of our meetings that no poor words of mine are needed to quicken your sense of loss, or to add to the poignancy of your regret.

The British Association has to deplore the loss from among us of Sir Joseph Gilbert, a veteran who continued to the end of a long life to pursue his important and beneficent researches with untiring energy. The length of his services in the cause of science cannot be better indicated than by recalling the fact that he was one of the six past presidents boasting fifty years' membership whose jubilee was celebrated by the Chemical Society in 1898. He was in fact an active member of that Society

for over sixty years. Early in his career he devoted himself to a most important but at that time little cultivated field of research. He strove with conspicuous success to place the oldest of industries on a scientific basis, and to submit the complex conditions of agriculture to a systematic analysis. He studied the physiology of plant life in the open air, not with the object of penetrating the secrets of structure, but with the more directly utilitarian aim of establishing the conditions of successful and profitable cultivation. By a long series of experiments alike well conceived and laboriously carried out, he determined the effects of variation in soil, and its chemical treatment—in short, in all the unknown factors with which the farmer previously had to deal according to empirical and local rules, roughly deduced from undigested experience by uncritical and rudimentary processes of inference. Gilbert had the faith, the insight, and the courage to devote his life to an investigation so difficult, so unpromising, and so unlikely to bring the rich rewards attainable by equal diligence in other directions, as to offer no attraction to the majority of men. The tabulated results of the Rothamsted experiments remain as a benefaction to mankind and a monument of indomitable and disinterested perseverance.

It is impossible for me in this place to offer more than the barest indication of the great place in contemporary science that has been vacated by the lamented death of Professor Alfred Cornu, who so worthily upheld the best traditions of scientific France. He was gifted in a high degree with the intellectual lucidity, the mastery of form, and the perspicuous methods which characterize the best exponents of French thought in all departments of study. After a brilliant career as a student, he was chosen at the early age of twenty-six to fill one of the enviable positions more numerous in Paris than in London, the professorship of physics at the Ecole Polytechnique. In that post, which he occupied to the end of his life, he found what is probably the ideal combination for a man of science—leisure and material equipment for original research, together with that close and stimulating contact with practical affairs afforded by his duties as teacher in a great school, almost ranking as a department of State. Cornu was admirable alike in the use he made of his opportunities and in his manner of discharging his duties. He was at once a great investigator and a great teacher. I shall not even attempt a summary, which at the best must be very imperfect, of his brilliant achievements in optics, the study of his predilection, in electricity, in acoustics, and in the field of physics generally. As a proof of the great estimation in which he was held, it is sufficient to remind you that he had filled the highest presidential offices in French scientific societies, and that he was a foreign member of our Royal Society and a recipient of its Rumford medal. In this country he had many friends, attracted no less by his personal and social qualities than by his commanding abilities. Some of those here present may remember his appearance a few years ago at the Royal Institution, and more recently his delivery of the Rede Lecture at Cambridge, when the University conferred upon him the honorary degree of Doctor of Science. His death has inflicted a heavy blow upon our generation, upon France, and upon the world.

THE PROGRESS OF BELFAST.

A great man has observed that the 'intelligent anticipation of events before they occur' is a factor of some importance in human affairs. One may suppose that intelligent anticipation had something to do

with the choice of Belfast as the meeting-place of the British Association this year. Or, if it had not, then it must be admitted that circumstances have conspired, as they occasionally do, to render the actual selection peculiarly felicitous. Belfast has perennial claims, of a kind that cannot easily be surpassed, to be the scene of a great scientific gathering—claims founded upon its scientific traditions and upon the conspicuous energy and success with which its citizens have prosecuted in various directions the application of science to the purposes of life. It is but the other day that the whole nation deplored at the grave of Lord Dufferin the loss of one of the most distinguished and most versatile public servants of the age. That great statesman and near neighbor of Belfast was a typical expression of the qualities and the spirit which have made Belfast what it is, and have enabled Ireland, in spite of all drawbacks, to play a great part in the Empire. I look around on your thriving and progressive city giving evidence of an enormous aggregate of industrial efforts intelligently organized and directed for the building up of a sound social fabric. I find that your great industries are interlinked and interwoven with the whole economic framework of the Empire, and that you are silently and irresistibly compelled to harmonious cooperation by practical considerations acting upon the whole community. It is here that I look for the real Ireland, the Ireland of the future. We cannot trace with precision the laws that govern the appearance of eminent men, but we may at least learn from history that they do not spring from every soil. They do not appear among decadent races or in ages of retrogression. They are the fine flower of the practical intellect of the nation working studiously and patiently in accordance with the great laws of conduct. In the manifold activities of Belfast we have a splendid manifestation of individual energy working necessarily, even if not altogether consciously, for the national good. In great Irishmen like Lord Dufferin and Lord Roberts, giving their best energies for the defense of the nation by diplomacy or by war, we have complementary evidence enough to reassure the most timid concerning the real direction of Irish energies and the vital nature of Irish solidarity with the rest of the Empire.

Belfast has played a prominent part in a transaction of a somewhat special and significant kind, which has proved not a little confusing and startling to the easy-going public. The significance of the shipping combination lies in the light it throws on the conditions and tendencies which make such things possible, if not even inevitable. It is an event forcibly illustrating the declaration of His Royal Highness the Prince of Wales, that the nation must 'wake up' if it hopes to face its growing responsibilities. Belfast may plead with some justice that it, at least, has never gone to sleep. In various directions an immense advance has been effected during the twenty-eight years that have elapsed since the last visit of the British Association. Belfast has become first a city and then a county, and now ranks as one of the eight largest cities in the United Kingdom. Its municipal area has been considerably extended, and its population has increased by something like seventy-five per cent. It has not only been extended, but improved and beautified in a manner which very few places can match, and which probably none can surpass. Fine new thoroughfares, adorned with admirable public institutions, have been run through areas once covered with crowded and squalid buildings. Compared with the early fifties, when iron shipbuilding was begun on a very modest scale, the customs collected

at the port have increased tenfold. Since the introduction of the power-loom, about 1850, Belfast has distanced all rivals in the linen industry, which continues to flourish notwithstanding the fact that most of the raw material is now imported, instead of being produced, as in former times, in Ulster. Extensive improvements have been carried out in the port at a cost of several millions, and have been fully justified by a very great expansion of trade. These few bare facts suffice to indicate broadly the immense strides taken by Belfast in the last two decades. For an Association that exists for the advancement of science it is stimulating and encouraging to find itself in the midst of a vigorous community, successfully applying knowledge to the ultimate purpose of all human effort, the amelioration of the common lot by an ever-increasing mastery of the powers and resources of Nature.

TYNDALL AND EVOLUTION.

The presidential address delivered by Tyndall in this city twenty-eight years ago will always rank as an epoch-making deliverance. Of all the men of the time, Tyndall was one of the best equipped for the presentation of a vast and complicated scientific subject to the mass of his fellowmen. Gifted with the powers of a many-sided original investigator, he had at the same time devoted much of his time to an earnest study of philosophy, and his literary and oratorical powers, coupled with a fine poetic instinct, were qualifications which placed him in the front rank of the scientific representatives of the later Victorian epoch, and constituted him an exceptionally endowed exponent of scientific thought. In the Belfast discourse Tyndall dealt with the changing aspects of the long unsettled horizon of human thought, at last illuminated by the sunrise of the doctrine of evolution. The consummate art with which he marshalled his scientific forces for the purpose of effecting conviction of the general truth of the doctrine has rarely been surpassed. The courage, the lucidity, the grasp of principles, the moral enthusiasm with which he treated his great theme, have powerfully aided in effecting a great intellectual conquest, and the victory assuredly ought to engender no regrets.

Tyndall's views as a strenuous supporter and believer in the theory of evolution were naturally essentially optimistic. He had no sympathy with the lugubrious pessimistic philosophy whose disciples are for ever intent on administering rebuke to scientific workers by reminding them that, however much knowledge man may have acquired, it is as nothing compared with the immensity of his ignorance. That truth is indeed never adequately realized except by the man of science, to whom it is brought home by repeated experience of the fact that his most promising excursions into the unknown are invariably terminated by barriers which, for the time at least, are insurmountable. He who has never made such excursions with patient labor may indeed prattle about the vastness of the unknown, but he does so without real sincerity or intimate conviction. His tacit, if not his avowed, contention is that since we can never know all it is not worth while to seek to know more; and that in the profundity of his ignorance he has the right to people the unexplored spaces with the phantoms of his vain imagining. The man of science, on the contrary, finds in the extent of his ignorance a perpetual incentive to further exertion, and in the mysteries that surround him a continual invitation, nay, more, an inexorable mandate. Tyndall's writings abundantly prove that he had faced the great problems of man's existence with that calm intellectual courage, the lack of

which goes very far to explain the nervous dogmatism of nescience. Just because he had done this, because he had, as it were, mapped out the boundaries between what is knowable though not yet known and what must remain forever unknowable to man, he did not hesitate to place implicit reliance on the progress of which man is capable, through the exercise of patient and persistent research. In Tyndall's scheme of thought the chief dicta were the strict division of the world of knowledge from that of emotion, and the lifting of life by throwing overboard the malign residuum of dogmatism, fanaticism and intolerance, thereby stimulating and nourishing a plastic vigor of intellect. His cry was 'Commotion before stagnation, the leap of the torrent before the stillness of the swamp.'

His successors have no longer any need to repeat those significant words, 'We claim and we shall wrest from theology the entire domain of cosmological theory.' The claim has been practically, though often unconsciously, conceded. Tyndall's dictum, 'Every system must be plastic to the extent that the growth of knowledge demands,' struck a note that was too often absent from the heated discussions of days that now seem so strangely remote. His honorable admission that, after all that had been achieved by the developmental theory, 'the whole process of evolution is the manifestation of a power absolutely inscrutable to the intellect of man,' shows how willingly he acknowledged the necessary limits of scientific inquiry. This reservation did not prevent him from expressing the conviction forced upon him by the pressure of intellectual necessity, after exhaustive consideration of the known relations of living things, that matter in itself must be regarded as containing the promise and potency of all terrestrial life. Bacon in his day said very much the same thing: 'He that will know the properties and proceedings of matter should comprehend in his understanding the sum of all things, which have been, which are, and which shall be, although no knowledge can extend so far as to singular and individual beings.' Tyndall's conclusion was at the time thought to be based on a too insecure projection into the unknown, and some even regarded such an expansion of the crude properties of matter as totally unwarranted. Yet Tyndall was certainly no materialist in the ordinary acceptation of the term. It is true his arguments, like all arguments, were capable of being distorted, especially when taken out of their context, and the address became in this way an easy prey for hostile criticism. The glowing rhetoric that gave charm to his discourse and the poetic similes that clothed the dry bones of his close-woven logic were attacked by a veritable broadside of critical artillery. At the present day these would be considered as only appropriate artistic embellishments, so great is the unconscious change wrought in our surroundings. It must be remembered that, while Tyndall discussed the evolutionary problem from many points of view, he took up the position of a practical disciple of Nature dealing with the known experimental and observational realities of physical inquiry. Thus he accepted as fundamental concepts the atomic theory, together with the capacity of the atom to be the vehicle or repository of energy, and the grand generalization of the conservation of energy. Without the former, Tyndall doubted whether it would be possible to frame a theory of the material universe; and as to the latter he recognized its radical significance in that the ultimate philosophical issues therein involved were as yet but dimly seen. That such generalizations are provisionally accepted does not mean that science is not alive to the possibility that what may now be regarded as fundamental

may in future be superseded or absorbed by a wider generalization. It is only the poverty of language and the necessity for compendious expression that oblige the man of science to resort to metaphor and to speak of the Laws of Nature. In reality, he does not pretend to formulate any laws for Nature, since to do so would be to assume a knowledge of the inscrutable cause from which alone such laws could emanate. When he speaks of a 'law of Nature' he simply indicates a sequence of events which, so far as his experience goes, is invariable, and which therefore enables him to predict, to a certain extent, what will happen in given circumstances. But, however seemingly bold may be the speculation in which he permits himself to indulge, he does not claim for his best hypothesis more than provisional validity. He does not forget that to-morrow may bring a new experience compelling him to recast the hypothesis of to-day. This plasticity of scientific thought, depending upon reverent recognition of the vastness of the unknown, is oddly made a matter of reproach by the very people who harp upon the limitations of human knowledge. Yet the essential condition of progress is that we should generalize to the best of our ability from the experience at command, treat our theory as provisionally true, endeavor to the best of our power to reconcile with it all the new facts we discover, and abandon or modify it when it ceases to afford a coherent explanation of new experience. That procedure is far as are the poles asunder from the presumptuous attempt to travel beyond the study of secondary causes. Any discussion as to whether matter or energy was the true reality would have appeared to Tyndall as a futile metaphysical disputation, which, being completely dissociated from verified experience, would lead to nothing. No explanation was attempted by him of the origin of the bodies we call elements, nor how some of such bodies came to be compounded into complex groupings and built up into special structures with which, so far as we know, the phenomena characteristic of life are invariably associated. The evolutionary doctrine leads us to the conclusion that life, such as we know it, has only been possible during a short period of the world's history, and seems equally destined to disappear in the remote future; but it postulates the existence of a material universe endowed with an infinity of powers and properties, the origin of which it does not pretend to account for. The enigma at both ends of the scale Tyndall admitted, and the futility of attempting to answer such questions he fully recognized. Nevertheless, Tyndall did not mean that the man of science should be debarred from speculating as to the possible nature of the simplest forms of matter or the mode in which life may have originated on this planet. Lord Kelvin, in his presidential address, put the position admirably when he said 'Science is bound by the everlasting law of honor to face fearlessly every problem that can fairly be presented to it. If a probable solution consistent with the ordinary course of Nature can be found, we must not invoke an abnormal act of Creative Power'; and in illustration he forthwith proceeded to express his conviction that from time immemorial many worlds of life besides our own have existed, and that 'it is not an unscientific hypothesis that life originated on this earth through the moss-grown fragments from the ruins of another world.' In spite of the great progress made in science, it is curious to notice the occasional recrudescence of metaphysical dogma. For instance, there is a school which does not hesitate to revive ancient mystifications in order to show that matter and energy can be shattered by philosophical arguments, and have no ob-

jective reality. Science is at once more humble and more reverent. She confesses her ignorance of the ultimate nature of matter, of the ultimate nature of energy, and still more of the origin and ultimate synthesis of the two. She is content with her patient investigation of secondary causes, and glad to know that since Tyndall spoke in Belfast she has made great additions to the knowledge of general molecular mechanism, and especially of synthetic artifice in the domain of organic chemistry, though the more exhaustive acquaintance gained only forces us the more to acquiesce in acknowledging the inscrutable mystery of matter. Our conception of the power and potency of matter has grown in little more than a quarter of a century to much more imposing dimensions, and the outlook for the future assuredly suggests the increasing acceleration of our rate of progress. For the impetus he gave to scientific work and thought, and for his fine series of researches chiefly directed to what Newton called the more secret and noble works of Nature within the corpuscles, the world owes Tyndall a debt of gratitude. It is well that his memory should be held in perennial respect, especially in the land of his birth.

THE ENDOWMENT OF EDUCATION.

These are days of munificent benefactions to science and education, which however are greater and more numerous in other countries than in our own. Splendid as they are, it may be doubted, if we take into account the change in the value of money, the enormous increase of population, and the utility of science to the builders of colossal fortunes, whether they bear comparison with the efforts of earlier days. But the habit of endowing science was so long in practical abeyance that every evidence of its resumption is matter for sincere congratulation. Mr. Cecil Rhodes has dedicated a very large sum of money to the advancement of education, though the means he has chosen are perhaps not the most effective. It must be remembered that his aims were political as much as educational. He had the noble and worthy ambition to promote enduring friendship between the great English-speaking communities of the world, and knowing the strength of college ties he conceived that this end might be greatly furthered by bringing together at an English university the men who would presumably have much to do in later life with the influencing of opinion, or even with the direction of policy. It has been held by some a striking tribute to Oxford that a man but little given to academic pursuits or modes of thought should think it a matter of high importance to bring men from our colonies or even from Germany, to submit to the formative influences of that ancient seat of learning. But this is perhaps reading Mr. Rhodes backwards. He showed his affectionate recollection of his college days by his gift to Oriel. But, apart from the main idea of fostering good relations between those who will presumably be influential in England, in the colonies and in the United States, Mr. Rhodes was probably influenced also by the hope that the influx of strangers would help to broaden Oxford notions and to procure revision of conventional arrangements.

Dr. Andrew Carnegie's endowment of Scottish universities, as modified by him in deference to expert advice, is a more direct benefit to the higher education. For while Mr. Rhodes has only enabled young men to get what Oxford has to give, Dr. Carnegie has also enabled his trustees powerfully to augment and improve the teaching equipment of the universities themselves. At the same time he has provided as far as possible for the enduring

usefulness of his money. His trustees form a permanent body external to the universities, which, while possessing no power of direct control, must always, as holder of the purse-strings, be in a position to offer independent and weighty criticisms. More recently Dr. Carnegie has devoted an equal sum of ten million dollars to the foundation of a Carnegie Institution in Washington. Here again he has been guided by the same ideas. He has neither founded a university nor handed over the money to any existing university. He has created a permanent trust charged with the duty of watching educational efforts and helping them from the outside according to the best judgment that can be formed in the circumstances of the moment. Its aims are to be—to promote original research; to discover the exceptional man in every department of study, whether inside or outside of the schools, and to enable him to make his special study his life-work; to increase facilities for higher education; to aid and stimulate the universities and other educational institutions; to assist students who may prefer to study at Washington; and to ensure prompt publication of scientific discoveries. The general purpose of the founder is to secure, if possible, for the United States leadership in the domain of discovery and the utilization of new forces for the benefit of man. Nothing will more powerfully further this end than attention to the injunction to lay hold of the exceptional man whenever and wherever he may be found, and, having got him, to enable him to carry on the work for which he seems specially designed. That means, I imagine, a scouring of the old world, as well as of the new, for the best men in every department of study—in fact, an assiduous collecting of brains similar to the collecting of rare books and works of art which Americans are now carrying on in so lavish a manner. As in diplomacy and war, so in science, we owe our reputation, and no small part of our prosperity, to exceptional men; and that we do not enjoy these things in fuller measure we owe to our lack of an army of well-trained ordinary men capable of utilizing their ideas. Our exceptional men have too often worked in obscurity, without recognition from a public too imperfectly instructed to guess at their greatness, without assistance from a State governed largely by dialecticians, and without help from academic authorities hidebound by the pedantries of medieval scholasticism. For such men we have to wait upon the will of Heaven. Even Dr. Carnegie will not always find them when they are wanted. But what can be done in that direction will be done by institutions like Dr. Carnegie's, and for the benefit of the nation that possesses them in greatest abundance and uses them most intelligently. When contemplating these splendid endowments of learning, it occurred to me that it would be interesting to find out exactly what some definite quantity of scientific achievement has cost in hard cash. In an article by Carl Snyder in the January number of the *North American Review*, entitled 'America's Inferior Place in the Scientific World,' I found the statement that 'it would be hardly too much to say that during the hundred years of its existence the Royal Institution alone has done more for English science than all of the English universities put together. This is certainly true with regard to British industry, for it was here that the discoveries of Faraday were made.' I was emboldened by this estimate from a distant and impartial observer to do what otherwise I might have shrunk from doing, and to take the Royal Institution—after all, the foundation of an American citizen, Count Rumford—as the basis of my inquiry. The work done

at the Royal Institution during the past hundred years is a fairly definite quantity in the mind of every man really conversant with scientific affairs. I have obtained from the books accurate statistics of the total expenditure on experimental inquiry and public demonstration for the whole of the nineteenth century. The items are:

Professors' salaries—physics and chemistry	£ 54,600
Laboratory expenditure	24,430
Assistants' salaries	21,590
Total for one hundred years	£100,620

In addition, the members and friends of the Institution have contributed to a fund for exceptional expenditure for Experimental Research the sum of 9,580*l.* It should also be mentioned that a Civil List pension of 300*l.* was granted to Faraday in 1853, and was continued during twenty-seven years of active work and five years of retirement. Thirty-two years in all, at 300*l.* a year, making a sum of 9,600*l.*, representing the national donation, which, added to the amount of expenditure just stated, brings up the total cost of a century of scientific work in the laboratories of the Royal Institution, together with public demonstrations, to 119,800*l.*, or an average of 1,200*l.* per annum. I think if you recall the names and achievements of Young, Davy, Faraday and Tyndall, you will come to the conclusion that the exceptional man is about the cheapest of natural products. It is a popular fallacy that the Royal Institution is handsomely endowed. On the contrary, it has often been in financial straits; and since its foundation by Count Rumford its only considerable bequests have been one from Thomas G. Hodgkins, an American citizen, for Experimental Research, and that of John Fuller for endowing with 95*l.* a year the chairs of Chemistry and Physiology. In this connection the Davy-Faraday Laboratory, founded by the liberality of Dr. Ludwig Mond, will naturally occur to many minds. But though affiliated to the Royal Institution, with, I hope, reciprocal indirect advantages, that Laboratory is financially independent and its endowments are devoted to its own special purpose, which is to provide opportunity to prosecute independent research for worthy and approved applicants of all nationalities. The main reliance of the Royal Institution has always been, and still remains, upon the contributions of its members, and upon corresponding sacrifices in the form of time and labor by its professors. It may be doubted whether we can reasonably count upon a succession of scientific men able and willing to make sacrifices which the conditions of modern life tend to render increasingly burdensome. Modern science is in fact in something of a dilemma. Devotion to abstract research upon small means is becoming always harder to maintain, while at the same time the number of wealthy independent searchers after truth and patrons of science of the style of Joule, Spottiswoode and De la Rue is apparently becoming smaller. The installations required by the refinements of modern science are continually becoming more costly, so that upon all grounds it would appear that without endowments of the kind provided by Dr. Carnegie the outlook for disinterested research is rather dark. On the other hand, these endowments, unless carefully administered, might obviously tend to impair the single-minded devotion to the search after truth for its own sake, to which science has owed almost every memorable advance made in the past. The Carnegie Institution will dispose in a year of as much money as the members of the Royal Institution have expended in a century upon its purely scientific work. It will at least be interesting to note how far the output

of high-class scientific work corresponds to the hundredfold application of money to its production. Nor will it be of less interest to the people of this country to observe the results obtained from that moiety of Dr. Carnegie's gift to Scotland which is to be applied to the promotion of scientific research.

APPLIED CHEMISTRY, ENGLISH AND FOREIGN.

The Diplomatic and Consular reports published from time to time by the Foreign Office are usually too belated to be of much use to business men, but they sometimes contain information concerning what is done in foreign countries which affords food for reflection. One of these reports, issued a year ago, gives a very good account of the German arrangements and provisions for scientific training, and of the enormous commercial demand for the services of men who have passed successfully through the universities and technical high schools, as well as of the wealth that has accrued to Germany through the systematic application of scientific proficiency to the ordinary business of life.

Taking these points in their order, I have thought it a matter of great interest to obtain a comparative view of chemical equipment in this country and in Germany, and I am indebted to Professor Henderson, of Glasgow, who last year became the secretary of a committee of this Association of which Professor Armstrong is chairman, for statistics referring to this country, which enable a comparison to be broadly made. The author of the consular report estimates that in 1901 there were 4,500 trained chemists employed in German works, the number having risen to this point from 1,700 employed twenty-five years earlier. It is difficult to give perfectly accurate figures for this country, but a liberal estimate places the number of works chemists at 1,500, while at the very outside it cannot be put higher than somewhere between 1,500 and 2,000. In other words, we cannot show in the United Kingdom, notwithstanding the immense range of the chemical industries in which we once stood prominent, more than one third of the professional staff employed in Germany. It may perhaps be thought or hoped that we make up in quality for our defect in quantity, but unfortunately this is not the case. On the contrary, the German chemists are, on the average, as superior in technical training and acquirements as they are numerically. Details are given in the report of the training of 633 chemists employed in German works. Of these, 69 per cent. hold the degree of Ph.D., about 10 per cent. hold the diploma of a technical high school, and about 5 per cent. hold both qualifications. That is to say 84 per cent. have received a thoroughly systematic and complete chemical training, and 74 per cent. of these add the advantages of a university career. Compare with this the information furnished by 500 chemists in British works. Of these only 21 per cent. are graduates, while about 10 per cent. hold the diploma of a college. Putting the case as high as we can, and ignoring the more practical and thorough training of the German universities, which give their degrees for work done, and not for questions asked and answered on paper, we have only 31 per cent. of systematically trained chemists against 84 per cent. in German works. It ought to be mentioned that about 21 per cent. of the 500 are fellows or associates of the Institute of Chemistry, whatever that may amount to in practice, but of these a very large number have already been accounted for under the heads of graduates and holders of diplomas. These figures, which I suspect are much too favorable on the British side, unmistakably point to the prevalence among employers in this coun-

try of the antiquated adherence to rule of thumb, which is at the root of much of the backwardness we have to deplore. It hardly needs to be pointed out to such an audience as the present that chemists who are neither graduates of a university, nor holders of a diploma from a technical college, may be competent to carry on existing processes according to traditional methods, but are very unlikely to effect substantial improvements, or to invent new and more efficient processes. I am very far from denying that hereand there an individual may be found whose exceptional ability enables him to triumph over all defects of training. But in all educational matters it is the average man whom we have to consider, and the average ability which we have to develop. Now, to take the second point—the actual money value of the industries carried on in Germany by an army of workers both quantitatively and qualitatively so superior to our own. The Consular report estimates the whole value of German chemical industries at not less than fifty millions sterling per annum. These industries have sprung up within the last seventy years, and have received enormous expansion during the last thirty. They are, moreover, very largely founded upon basic discoveries made by English chemists, but never properly appreciated or scientifically developed in the land of their birth. I will place before you some figures showing the growth of a single firm engaged in a single one of these industries—the utilization of coal tar for the production of drugs, perfumes and coloring-matters of every conceivable shade. The firm of Friedrich Bayer & Co. employed in 1875, 119 workmen. The number has more than doubled itself every five years, and in May of this year that firm employed 5,000 workmen, 160 chemists, 260 engineers and mechanics, and 680 clerks. For many years past it has regularly paid 18 per cent. on the ordinary shares, which this year has risen to 20 per cent.; and in addition, in common with other and even larger concerns in the same industry, has paid out of profits for immense extensions usually charged to capital account. There is one of these factories, the works and plant of which stand in the books at 1,500,-000*l.*, while the money actually sunk in them approaches to 5,000,000*l.* In other words, the practical monopoly enjoyed by the German manufacturers enables them to exact huge profits from the rest of the world, and to establish a position which, financially as well as scientifically, is almost unassailable. I must repeat that the fundamental discoveries upon which this gigantic industry is built were made in this country, and were practically developed to a certain extent by their authors. But in spite of the abundance and cheapness of the raw material, and in spite of the evidence that it could be most remuneratively worked up, these men founded no school and had practically no successors. The colors they made were driven out of the field by newer and better colors made from their stuff by the development of their ideas, but these improved colors were made in Germany and not in England. Now what is the explanation of this extraordinary and disastrous phenomenon? I give it in a word—want of education. We had the material in abundance when other nations had comparatively little. We had the capital, and we had the brains, for we originated the whole thing. But we did not possess the diffused education without which the ideas of men of genius cannot fructify beyond the limited scope of an individual. I am aware that our patent laws are sometimes held responsible. Well, they are a contributory cause; but it must be remembered that other nations with patent laws as protective as could be desired have not

developed the color industry. The patent laws have only contributed in a secondary degree, and if the patent laws have been bad the reason for their badness is again want of education. Make them as bad as you choose, and you only prove that the men who made them, and the public whom these men try to please, were misled by theories instead of being conversant with fact and logic. But the root of the mischief is not in the patent laws or in any legislation whatever. It is in the want of education among our so-called educated classes, and secondarily among the workmen on whom these depend. It is in the abundance of men of ordinary plodding ability, thoroughly trained and methodically directed, that Germany at present has so commanding an advantage. It is the failure of our schools to turn out, and of our manufacturers to demand, men of this kind, which explains our loss of some valuable industries and our precarious hold upon others. Let no one imagine for a moment that this deficiency can be remedied by any amount of that technical training which is now the fashionable nostrum. It is an excellent thing, no doubt, but it must rest upon a foundation of general training. Mental habits are formed for good or evil long before men go to the technical schools. We have to begin at the beginning: we have to train the population from the first to think correctly and logically, to deal at first hand with facts, and to evolve, each one for himself, the solution of a problem put before him, instead of learning by rote the solution given by somebody else. There are plenty of chemists turned out, even by our universities, who would be of no use to Bayer & Co. They are chockfull of formulæ, they can recite theories, and they know text-books by heart; but put them to solve a new problem, freshly arisen in the laboratory, and you will find that their learning is all dead. It has not become a vital part of their mental equipment, and they are floored by the first emergence of the unexpected. The men who escape this mental barrenness are men who were somehow or other taught to think long before they went to the university. To my mind, the really appalling thing is not that the Germans have seized this or the other industry, or even that they may have seized upon a dozen industries. It is that the German population has reached a point of general training and specialized equipment which it will take us two generations of hard and intelligently directed educational work to attain. It is that Germany possesses a national weapon of precision which must give her an enormous initial advantage in any and every contest depending upon disciplined and methodized intellect.

HISTORY OF COLD AND THE ABSOLUTE ZERO.

It was Tyndall's good fortune to appear before you at a moment when a fruitful and comprehensive idea was vivifying the whole domain of scientific thought. At the present time no such broad generalization presents itself for discussion, while on the other hand the number of specialized studies has enormously increased. Science is advancing in so broad a front by the efforts of so great an army of workers that it would be idle to attempt within the limits of an address to the most indulgent of audiences anything like a survey of chemistry alone. But I have thought it might be instructive, and perhaps not uninteresting, to trace briefly in broad outline the development of that branch of study with which my own labors have been recently more intimately connected—a study which I trust I am not too partial in thinking is as full of philosophical interest as of experimental difficulty. The nature of heat and cold must have engaged think-

ing men from the very earliest dawn of speculation upon the external world; but it will suffice for the present purpose if, disregarding ancient philosophers and even mediæval alchemists, we take up the subject where it stood after the great revival of learning, and as it was regarded by the father of the inductive method. That this was an especially attractive subject to Bacon is evident from the frequency with which he recurs to it in his different works, always with lamentation over the inadequacy of the means at disposal for obtaining a considerable degree of cold. Thus in the chapter in the 'Natural History,' 'Sylva Sylvarum,' entitled 'Experiments in consort touching the production of cold,' he says, 'The production of cold is a thing very worthy of the inquisition both for the use and the disclosure of causes. For heat and cold are nature's two hands whereby she chiefly worketh, and heat we have in readiness in respect of the fire, but for cold we must stay till it cometh or seek it in deep caves or high mountains, and when all is done we cannot obtain it in any degree, for furnaces of fire are far hotter than a summer sun, but vaults and hills are not much colder than a winter's frost.' The great Robert Boyle was the first experimentalist who followed up Bacon's suggestion. In 1682 Boyle read a paper to the Royal Society on 'New Experiments and Observations touching Cold, or an Experimental History of Cold,' published two years later in a separate work. This is really a most complete history of everything known about cold up to that date, but its great merit is the inclusion of numerous experiments made by Boyle himself on frigorific mixtures, and the general effects of such upon matter. The agency chiefly used by Boyle in the conduct of his experiments was the glaciating mixture of snow or ice and salt. In the course of his experiment he made many important observations. Thus he observed that the salts which did not help the snow or ice to dissolve faster gave no effective freezing. He showed that water in becoming ice expands by about one ninth of its volume, and bursts gun-barrels. He attempted to counteract the expansion and prevent freezing by completely filling a strong iron ball with water before cooling; anticipating that it might burst the bottle by the stupendous forces of expansion, or that if it did not, then the ice produced might under the circumstances be heavier than water. He speculated in an ingenious way on the change of water into ice. Thus he says, 'If cold be but a privation of heat through the recess of that ethereal substance which agitated the little eel-like particles of the water and thereby made them compose a fluid body, it may easily be conceived that they should remain rigid in the postures in which the ethereal substances quitted them, and thereby compose an unfluid body like ice; yet how these little eels should by that recess acquire as strong an endeavor outwards as if they were so many little springs and expand themselves with so stupendous a force, is that which does not so readily appear.' The greatest degree of adventitious cold Boyle was able to produce did not make air exposed to its action lose a full tenth of its own volume, so that, in his own words, the cold does not 'weaken the spring by anything near so considerable as one would expect.' After making this remarkable observation and commenting upon its unexpected nature, it is strange Boyle did not follow it up. He questions the existence of a body of its own nature supremely cold, by participating in which all other bodies obtain that quality, although the doctrine of a *primum frigidum* had been accepted by many sects of philosophers; for, as he says, 'if a body being cold signify no more than its not having its sensible parts so much agitated

as those of our sensorium, it suffices that the sun or the fire or some other agent, whatever it were, that agitated more vehemently its parts before, does either now cease to agitate them or agitates them but very remissly, so that till it be determined whether cold be a positive quality or but a privative it will be needless to contend what particular body ought to be esteemed the *primum frigidum*.' The whole elaborate investigation cost Boyle immense labor, and he confesses that he 'never handled any part of natural philosophy that was so troublesome and full of hardships.' He looked upon his results but as a 'beginning' in this field of inquiry, and for all the trouble and patience expended he consoled himself with the thought of 'men being oftentimes obliged to suffer as much wet and cold and dive as deep to fetch up sponges as to fetch up pearls.' After the masterly essay of Boyle, the attention of investigators was chiefly directed to improving thermometrical instruments. The old air thermometer of Galileo being inconvenient to use, the introduction of fluid thermometers greatly aided the inquiry into the action of heat and cold. For a time great difficulty was encountered in selecting proper fixed points on the scales of such instruments, and this stimulated men like Huygens, Newton, Hooke and Amontons to suggest remedies and to conduct experiments. By the beginning of the eighteenth century the freezing-point and the boiling-point of water were agreed upon as fixed points, and the only apparent difficulties to be overcome were the selection of the fluid, accurate calibration of the capillary tube of the thermometer, and a general understanding as to scale divisions. It must be confessed that great confusion and inaccuracy in temperature observations arose from the variety and crudeness of the instruments. This led Amontons in 1702–3 to contribute two papers to the French Academy which reveal great originality in the handling of the subject, and which, strange to say, are not generally known. The first discourse deals with some new properties of the air and the means of accurately ascertaining the temperature in any climate. He regarded heat as due to a movement of the particles of bodies, though he did not in any way specify the nature of the motion involved; and as the general cause of all terrestrial motion, so that in its absence the earth would be without movement in its smallest parts. The new facts he records are observations on the spring or pressure of air brought about by the action of heat. He shows that different masses of air measured at the same initial spring or pressure, when heated to the boiling-point of water, acquire equal increments of spring or pressure, provided the volume of the gas be kept at its initial value. Further, he proves that if the pressure of the gas before heating be doubled or tripled, then the additional spring or pressure resulting from heating to the boiling-point of water is equally doubled or tripled. In other words, the ratio of the total spring of air at two definite and steady temperatures and at constant volume is a constant, independent of the mass or the initial pressure of the air in the thermometer. These results led to the increased perfection of the air thermometer as a standard instrument, Amontons' idea being to express the temperature at any locality in fractions of the degree of heat of boiling water. The great novelty of the instrument is that temperature is defined by the measurement of the length of a column of mercury. In passing, he remarks that we do not know the extreme of heat and cold, but that he has given the results of experiments which establish correspondences for those who wish to consider the subject. In the following year

Amontons contributed to the Academy a further paper extending the scope of the inquiry. He there pointed out more explicitly that as the degrees of heat in his thermometer are registered by the height of a column of mercury, which the heat is able to sustain by the spring of the air, it follows that the extreme cold of the thermometer will be that which reduces the air to have no power of spring. This, he says, will be a much greater cold than what we call 'very cold,' because experiments have shown that if the spring of the air at boiling-point is 73 inches, the degree of heat which remains in the air when brought to the freezing-point of water is still very great, for it can still maintain the spring of 51½ inches. The greatest climatic cold on the scale of units adopted by Amontons is marked 50, and the greatest summer heat 58, the value for boiling water being 73, and the zero being 52 units below the freezing-point. Thus Amontons was the first to recognize that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature, and his scale is nothing else than the absolute one we are now so familiar with. It results from Amontons' experiment that the air would have no spring left if it were cooled below the freezing-point of water to about 2½ times the temperature range which separates the boiling-point and the freezing-point. In other words, if we adopt the usual centennial difference between these two points of temperature as 100 degrees, then the zero of Amontons' air thermometer is *minus* 240 degrees. This is a remarkable approximation to our modern value for the same point of *minus* 273 degrees. It has to be confessed that Amontons' valuable contributions to knowledge met with that fate which has so often for a time overtaken the work of too-advanced discoverers; in other words, it was simply ignored, or in any case not appreciated by the scientific world either of that time or half a century later. It is not till Lambert, in his work on 'Pyrometrie' published in 1779, repeated Amontons' experiments and endorsed his results that we find any further reference to the absolute scale or the zero of temperature. Lambert's observations were made with the greatest care and refinement, and resulted in correcting the value of the zero of the air scale to *minus* 270 degrees as compared with Amontons' *minus* 240 degrees. Lambert points out that the degree of temperature which is equal to zero is what one may call absolute cold, and that at this temperature the volume of the air would be practically nothing. In other words, the particles of the air would fall together and touch each other and become dense like water; and from this it may be inferred that the gaseous condition is caused by heat. Lambert says that Amontons' discoveries had found few adherents because they were too beautiful and advanced for the time in which he lived.

About this time a remarkable observation was made by Professor Braun at Moscow, who, during the severe winter of 1759, succeeded in freezing mercury by the use of a mixture of snow and nitric acid. When we remember that mercury was regarded as quite a peculiar substance possessed of the essential quality of fluidity, we can easily understand the universal interest created by the experiment of Braun. This was accentuated by the observations he made on the temperature given by the mercury thermometer, which appeared to record a temperature as low as *minus* 200° C. The experiments were soon repeated by Hutchins at Hudson's Bay, who conducted his work with the aid of suggestions given him by Cavendish and Black. The result of the new observations was to show that the freezing-point of mercury is only *minus* 40° C., the errors

in former experiments having been due to the great contraction of the mercury in the thermometer in passing into the solid state. From this it followed that the enormous natural and artificial colds which had generally been believed in had no proved existence. Still the possible existence of a zero of temperature very different from that deduced from gas thermometry had the support of such distinguished names as those of Laplace and Lavoisier. In their great memoir on 'Heat,' after making what they consider reasonable hypotheses as to the relation between specific heat and total heat, they calculate values for the zero which range from 1,500° to 3,000° below melting ice. On the whole, they regard the absolute zero as being in any case 600° below the freezing-point. Lavoisier, in his 'Elements of Chemistry' published in 1792, goes further in the direction of indefinitely lowering the zero of temperature when he says, 'We are still very far from being able to produce the degree of absolute cold, or total deprivation of heat, being unacquainted with any degree of coldness which we cannot suppose capable of still further augmentation; hence it follows we are incapable of causing the ultimate particles of bodies to approach each other as near as possible, and thus these particles do not touch each other in any state hitherto known.' Even as late as the beginning of the nineteenth century we find Dalton, in his new system of 'Chemical Philosophy,' giving ten calculations of this value, and adopting finally as the natural zero of temperature *minus* 3,000° C.

In Black's lectures we find that he takes a very cautious view with regard to the zero of temperature, but as usual is admirably clear with regard to its exposition. Thus he says, "We are ignorant of the lowest possible degree or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to what we should have of a chain, the two ends of which were hidden from us and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest according as they are nearest to or further removed from the principal links; but not knowing the distance of any links from the end of the chain we could not compare them togther with respect to their distance or say that one link was twice as far from the end of the chain as another." It is interesting to observe, however, that Black was evidently well acquainted with the work of Amontons and strongly supports his inference as to the nature of air. Thus, in discussing the general cause of vaporization, Black says that some philosophers have adopted the view "that every palpable elastic fluid in nature is produced and preserved in this form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences, at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity and condensed and even frozen into a solid matter were it in our power to apply to it a sufficient cold; that it is a substance that differs from others by being incomparably more volatile, and which is therefore converted into vapor and preserved in that form by a weaker heat than any that ever happened or can obtain in this globe, and which, therefore, cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present." The views that Black attributes to Amontons have been generally associated with the name of Lavoisier, who practically admitted similar possibilities as to the nature of air; but it is not likely that in such matters Black would commit any mistake as

to the real author of a particular idea, especially in his own department of knowledge. Black's own special contribution to low-temperature studies was his explanation of the interaction of mixtures of ice with salts and acids by applying the doctrine of the latent heat of fluidity of ice to account for the frigorific effect. In a similar way Black explained the origin of the cold produced in Cullen's remarkable experiment of the evaporation of ether under the receiver of an air-pump by pointing out that the latent heat of vaporization in this case necessitated such a result. Thus, by applying his own discoveries of latent heat, Black gave an intelligent explanation of the cause of all the low-temperature phenomena known in his day.

After the gaseous laws had been definitely formulated by Gay-Lussac and Dalton, the question of the absolute zero of temperature, as deduced from the properties of gases, was revived by Clement and Desormes. These distinguished investigators presented a paper on the subject to the French Academy in 1812, which, it appears, was rejected by that body. The authors subsequently elected to publish it in 1819. Relying on what we know now to have been a faulty hypothesis, they deduced from observations on the heating of air rushing into a vacuum the temperature of *minus* 267 degrees as that of the absolute zero. They further endeavored to show, by extending to lower temperatures the volume or the pressure coefficients of gases given by Gay-Lussac, that at the same temperature of *minus* 267 degrees the gases would contract so as to possess no appreciable volume, or, alternatively, if the pressure was under consideration, it would become so small as to be non-existent. Although full reference is given to previous work bearing on the same subject, yet, curiously enough, no mention is made of the name of Amontons. It certainly gave remarkable support to Amontons' notion of the zero to find that simple gases like hydrogen and compound gases like ammonia, hydrochloric, carbonic and sulphurous acids should all point to substantially the same value for this temperature. But the most curious fact about this research of Clement and Desormes is that Gay-Lussac was a bitter opponent of the validity of the inferences they drew either from his work or their own. The mode in which Gay-Lussac regarded the subject may be succinctly put as follows: A quick compression of air to one fifth volume raises its temperature to 300 degrees, and if this could be made much greater and instantaneous the temperature might rise to 1,000 or 2,000 degrees. Conversely, if air under five atmospheres were suddenly dilated, it would absorb as much heat as it had evolved during compression, and its temperature would be lowered by 300 degrees. Therefore, if air were taken and compressed to fifty atmospheres or more, the cold produced by its sudden expansion would have no limit. In order to meet this position Clement and Desormes adopted the following reasoning: They pointed out that it had not been proved that Gay-Lussac was correct in his hypothesis, but that in any case it tacitly involves the assumption that a limited quantity of matter possesses an unlimited supply of heat. If this were the case, then heat would be unlike any other measurable thing or quality. It is, therefore, more consistent with the course of nature to suppose that the amount of heat in a body is like the quantity of elastic fluid filling a vessel, which, while definite in original amount, one may make less and less by getting nearer to a complete exhaustion. Further, to realize the absolute zero in the one case is just as impossible as to realize the absolute vacuum in the other; and as we do not doubt a zero of pressure,

although it is unattainable, for the same reason we ought to accept the reality of the absolute zero. We know now that Gay-Lussac was wrong in supposing the increment of temperature arising from a given gaseous compression would produce a corresponding decrement from an identical expansion. After this time the zero of temperature was generally recognized as a fixed ideal point, but in order to show that it was hypothetical a distinction was drawn between the use of the expressions, zero of absolute temperature and the absolute zero.

The whole question took an entirely new form when Lord Kelvin, in 1848, after the mechanical equivalent of heat had been determined by Joule, drew attention to the great principles underlying Carnot's work on the 'Motive Power of Heat,' and applied them to an absolute method of temperature measurement, which is completely independent of the properties of any particular substance. The principle was that for a difference of one degree on this scale, between the temperatures of the source and refrigerator, perfect engine should give the same amount of work in every part of the scale. Taking the same fixed points as for the Centigrade scale, and making 100 of the new degrees cover that range, it was found that the degrees not only within that range, but as far beyond as experimental data supplied the means of comparison, differed by only minute quantities from those of Regnault's air thermometer. The zero of the new scale had to be determined by the consideration that when the refrigerator was at the zero of temperature the perfect engine should give an amount of work equal to the full mechanical equivalent of the heat taken up. This led to a zero of 273 degrees below the temperature of freezing water, substantially the same as that deduced from a study of the gaseous state. It was a great advance to demonstrate by the application of the laws of thermodynamics not only that the zero of temperature is a reality, but that it must be located at 273 degrees below the freezing-point of water. As no one has attempted to impugn the solid foundation of theory and experiment on which Lork Kelvin based his thermodynamic scale, the existence of a definite zero of temperature must be acknowledged as a fundamental scientific fact.

JAMES DEWAR.

(To be concluded.)

SCIENTIFIC BOOKS.

Essays in Historical Chemistry. By T. E. THORPE, LL.D., F.R.S., Principal of the Government Laboratory, London. London and New York, Macmillan Co. 1902. 8vo. Pp. 582.

This book, as explained in the preface, consists mainly of lectures and addresses given at various times to audiences of very different type during the last twenty-five years. Although the author says his book has no pretensions to be considered a history of chemistry, even of the time to which the narratives relate, it is in reality a most interesting and charmingly written account of chemical discovery and of the development of chemical theory of the past century as connected with the lives of the great men who have made the science of chemistry what it is to-day.

It is true that none of these essays deals directly with Black, Dalton, Berzelius or Liebig, yet there is so much incidental mention of the work of these investigators that their places in the growing science are amply indicated.

Boyle, Priestley, Cavendish, Watt, Faraday and Graham are the English subjects of these addresses, and from the Continent we have Scheele, Lavoisier, Wöhler, Dumas, Kopp, Victor Meyer, Mendeleeff and Cannizzaro, and the latter group are as sympathetically treated as the former.

The author has the happy gift of making the subjects of his study stand out vividly as individuals, and we follow their careers,

from their student days to the high positions which they all attained, with an interest which never flags. The personal relations of those who were contemporaries are also happily stated, and the book, as a whole, gives us a living picture of the growth of chemical science which differs, most fortunately, from most of the systematic treatises on the history of chemistry.

In the controversial address, inspired by Berthelot's 'La Révolution Chimique,' in which he claimed for Lavoisier the right to the discovery and coordination of those general ideas relating to the composition of air and water, Dr. Thorpe is a sturdy and convincing defender of the claims of Priestley and Cavendish. And yet we cannot help feeling that his task would have been an easier one if the English chemists had not held on so tenaciously to the fantastical idea of phlogiston, which prevented them from grasping the true and simple relation of oxygen and nitrogen in air and oxygen and hydrogen in water. That this controversy does not blind the author to seeing Lavoisier in his true position as the founder of modern chemistry is shown in his article on Lavoisier in the *Contemporary Review* of December, 1900, in which he speaks of him as 'the dominant figure in the chemical world of the last century.'

The addresses are all of such great interest and value that it is not easy to select one or more of especial merit. And yet it is perhaps noticeable that the author is most attracted by the personality of Graham among the English chemists and of Wöhler, Kopp and Victor Meyer among the German. Admirable they all are, and well worthy of collection in the permanent form now before us.

The concluding addresses on 'The Rise and Development of Synthetic Chemistry,' 'On the Progress of Chemistry in Great Britain and Ireland during the Nineteenth Century,' and 'On the Development of the Chemical Arts during the Reign of Queen Victoria' are worthy of a place in the volume, but they lack the life of the addresses which deal with the personality of the masters of the science.

The history of chemistry is not often successfully taught in our technical schools, for the reason, perhaps, that not many teachers are able to make it interesting. With this collection of essays as a basis for reading, the average teacher would find his students much more receptive of systematic instruction in the subject.

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SCIENTIFIC JOURNALS AND ARTICLES.

Contents of September, 1902, number of *Terrestrial Magnetism and Atmospheric Electricity:*

Portrait of General Sir Edward Sabine, Frontispiece; 'Ueber Die Meteorologische Natur der Variationen des Erdmagnetismus,' A. Nippoldt; 'Work in Terrestrial Magnetism and Atmospheric Electricity in South Africa,' J. C. Beattie; 'Wilson and Gibbs's Vector Analysis,' E. W. Hyde; 'Note Sur L'Amplitude de L'Oscillation Diurne de la Déclinaison Magnetique et Son Inégalité Mensuelle,' J. de Moidrey; 'Note Sur la Variation Séculaire de la Déclinaison à Zi-ka-wei (Chine), J. de Moidrey; 'Biographical Sketch of General Sir Edward Sabine, F.R.S., K.C.B.'; 'Magnetic Deflection of Long Steel Wire Plumb-lines,' W. Hallock; 'Divergence of Long Plumb-lines at the Tamarack Mine,' F. W. McNair; Notes, Abstracts and Reviews, Recent Publications.

DISCUSSION AND CORRESPONDENCE.

INVESTIGATION VERSUS ERUDITION.

To the Editor of Science: It is very natural and desirable that scientific men of experience should give counsel upon the education of those to whom their labors, finished and unfinished, must be bequeathed. On the other hand, it will be a misfortune if they who have surrounded themselves with facilities for investigating their respective subjects forget the condition of the beginner and mislead him either with vain hopes or with unwarranted discouragement. Both these dangers seem to inhere in a proposition advanced in many of the addresses before scientific bodies, with which the columns of Science abound. Professor Thurston's able paper furnishes a recent and excellent example of the bogus educational axiom to which exception is taken.

Scientific research is the highest work undertaken by the man of science, and it can be undertaken with confidence only by him who has made himself familiar with the state of his art, to date, or by the genius whose inspiration may, now and then, make learning, for the time and occasion, less essential.

* * * The first step is thus the acquirement of a complete knowledge of the essential work of investigation which has been accomplished by others to date. This eliminates the primary work and permits avoidance of repetition, as well as reveals the suggestions of every great mind which has attacked the problem in its preliminary stages, and places the investigation on the level from which further advance becomes directly and effectively practicable. It also gives the proposing investigator a firm and ample foundation on which to build higher and exhibits to him the trend of the work, in advance.*

Researches directed toward the increase of detailed knowledge might be contrasted with those concerned with generalization. Professor Thurston's argument would apply to the former far better than to the latter.

That we should first learn everything known about a subject before trying to find out anything new may appear self-evident, but it is no more true for young investigators than for ordinary students whom we are now at such pains to instruct by 'laboratory methods.' The investigator who has become familiar with a specific problem may sometimes obtain valuable suggestions from the failures of his predecessors, but to canvass all the literature of a department of research may not only involve an enormous waste of time and energy, but does not constitute a preparation for the work of investigation. The academic simpleton will, of course, consider this the same as to allege that the more ignorant one may be the better he can investigate, but there is a difference which patient analysis may enable him to appreciate.

'A little learning is a dangerous thing,' and more is more dangerous. For learning, as such, the investigator has no use. Knowledge is valuable to him—the more the better—but it is as suicidal folly for him to cumber his brain with a miscellaneous assortment of the observations and theories of others as for the athlete to surfeit his stomach before a foot-race.

The first and most essential preliminary for a successful investigation is an interest in the question, and any method of procedure which tends to diminish or relax interest is false and futile. Diligence in learning the facts of a science is a distinctly unfavorable symptom in a would-be investigator when unaccompanied by a vital constructive interest. That a student hoards facts does not mean that he will build anything with them. Intellectual misers are common, and are quite as unprofitable as the monetary variety. A scientific specialist may have vast knowledge and life-long experience, and yet may never entertain an original idea or make a new rift in the wall of the unknown which baffled his predecessors. Indeed, such men commonly resent a readjustment of the bounds of knowledge as an interference with their vested capital of erudition.

Investigation is a sentiment, an instinct, a habit of mind; it is man's effort at knowing and enjoying the universe. The productive investigator desires knowledge for a purpose; he may not be eager for knowledge in general, nor for new knowledge in particular. He values details for their bearing upon the problem he hopes to solve. He can gather and sift them to advantage only in the light of a radiant interest, and his ability to utilize them for correct inferences depends on the delicacy of his perception and the strength of his mental grasp. The trainers put the athletes on a restricted diet with copious practice, but the efforts of the professors are directed toward the production of a flabby intellectual corpulence.

The investigator, like the athlete, must first be born; he can not be made to order, but his training determines the degree of excellence to which he can attain. No amount of training can remove organic defects, but bad training may be worse than none in lessening the attainment of the most capable. That education is false and injurious which puts the matter first and retards or prevents the development of constructive mental ability, a

*'Scientific Research: The Art of Revelation and of Prophecy,' SCIENCE, N. S. Vol. XVI., pp. 401–409, September 12, 1902.

power not peculiar to the investigator, but in him reaching the greatest scope and freedom of action.

The investigator must not only be born, he must be permitted to grow up. He needs nourishing food, but equally needs to retain the power of securing and digesting it for himself. Twenty years is long enough to acquire or lose any habit, and it is not strange that after a youth consumed in our modern and efficient system of kindergartens, primary, grammar and high schools, colleges and universities, the graduate, and even the postgraduate, continues to expect somebody to tell him what to do next. In Germany it has been found necessary to offset the goose-liver-stuffing experience of the primary schools and gymnasia by a return to social barbarism in the university, but the self-assertion secured through rowdyism and immorality is no true substitute for the lost integrity of the intellect. The German's confidence in a highly developed governmental and educational machinery gives him little opportunity to perceive what is very apparent in our pioneer country where a large proportion of productive investigators have not suffered the disadvantage of too intensive education. Many are not even college men, and of those who are many come from small, poorly equipped institutions whose intellectual and social demands did not completely monopolize the time and interest of the period of intellectual growth. These men did not take their colleges too seriously, and did not cease to feel responsible for their own intellectual salvation. Modern philanthropy has reared palaces of learning in which all the supposed needs of the human mind are anticipated and supplied; the question now is whether an endowed education has not the same dangers as an endowed religion.

O. F. Cook.

Washington, September 22, 1902.

SHORTER ARTICLES.

PREPOTENCY IN POLYDACTYLOUS CATS.

It has long been one of the common notions in post-Darwinian speculations that the variations which produce new species have small beginnings and increase very gradually, variations sufficiently striking to be classed as sports being considered practically incapable of modifying the species, since the number of individuals with the same abnormality would be relatively small, and the abnormal variation would be swamped by a few generations of crossing with normal, that is, average individuals. This notion seems to be based on the assumption that the characters of the offspring are the average of the characters of the two parents—that, in other words, an abnormality in either parent (the other being normal) is reduced one half in each succeeding generation.

The following observations, however, do not support this view. Not only do abnormal variations persist from generation to generation, but they even become more conspicuous, although one parent is always normal. The facts accord with Poulton's observations on a family of polydactylous cats (*Nature*, 1883 and 1887).

Some weeks ago my attention was called to three generations of cats in the possession of a Los Angeles family, many of the cats being furnished with an abnormal number of toes on both manus and pes. All are descended from a stray female of unknown pedigree, which possessed twenty-two toes, six (instead of five) on each manus, and five (instead of four) on each pes. This female, crossing with normal males, has produced several litters. In one litter there were five kittens, four of which were normal, the other having the normal number of five toes on each manus, but not the normal arrangement, the hallux being on a line with the others and equalling them in size. Each pes had six toes. The phalanges were apparently well formed, the same number to every toe.

Another litter contained several abnormal kittens (no accurate account was kept of the ratio of normal to abnormal), one of which survives and has been examined by me, as have all the other abnormal cats to be mentioned. It has six toes on the right manus, seven on the left manus, and the normal number, four, on each pes. Such a condition may be represented in the following manner:

7	6
4	4

This cat is a female and has borne three litters with normal fathers. Fifty per cent., as the owners remembered, were abnormal in one litter. The sole survivor has twenty-four toes, six on each manus and pes, all practically equivalent. In another litter more than fifty per cent. were abnormal; the sole survivor, a male, has the digit formula of

$$\begin{array}{c|c} 7 & 6 \\ \hline 6 & 6 \end{array}$$

with a total of twenty-five toes. In the third litter there are five kittens. Three are abnormal, with the following formulæ:

$$\begin{array}{c|c} 6 & 6 \\ \hline 5 & 5 \end{array}, \quad \begin{array}{c|c} 6 & 6 \\ \hline 4 & 4 \end{array}, \quad \begin{array}{c|c} 7 & 7 \\ \hline 5 & 5 \end{array}.$$

The last formula represents the number of digits when the kitten was a few days old. The first (inside) digit on one pes has now totally disappeared, and the corresponding one on the other pes is fast shriveling away; so that the normal number on each pes is being secondarily established by a resorption of No. 1, the toe which is normally absent on the pes and reduced on the manus.

In each of the four instances in which seven toes appear on one foot they are arranged in two groups. Toes Nos. 7, 6, 5, 4 (7 being the outermost toe) resemble the main four toes of the normal manus (*i. e.*, 5, 4, 3, 2). Of the three constituting the second group No. 2 is larger than any of the other six toes. Nos. 1 and 3 are of about equal size and smaller than any of the other five. Nos. 1, 2 and 3, taken together, seem to form a second (supernumerary) foot. It is interesting that seven toes occur only on a manus, which had normally more toes than the pes. The fact that the fifth toe degenerated in one case on the inside of the pes indicates that the supernumerary toes are added on the inside of the foot. This probably does not hold when there are two supernumeraries on the manus (seven in all), where, as Poulton held, the innermost toe may represent the hallux, or the supernumeraries may be interpreted as Freeland Howe, Jr., has recently (*Am. Nat.*, July, 1902) interpreted them in six-toed feet. According to this interpretation the outermost three toes are comparable to digits 3, 4, 5, of the normal pes. None of the other three individually represent Nos. 1 or 2, but collectively they replace No. 1 plus No. 2. This seems to me the more probable view in the present instance.

A review of the above facts shows the marked prepotency of the sport. The grandmother (generation I.) had

$$\begin{array}{c|c} 6 & 6 \\ \hline 5 & 5 \end{array}$$

or 22 toes. In generation II., one litter contained but one abnormal kitten among five (twenty per cent.), with a total of 22 toes. The other litter contained several abnormal ones, the sole survivor possessing seven toes on one manus, though with a total of but 21. From this cat have arisen the three litters of generation III., in which one has 25 toes (one manus having 7), two have 24 (one of these having 7 on each manus), and all three litters possessed not less than fifty per cent. of abnormal individuals, the last having sixty per cent. It is clear that the total number as well as the number on each manus and pes is increasing from generation to generation.

There seems to be a no less remarkable prepotency of sex. The male cat with 25 toes, when crossed with normal females, seems to have had no influence on the number of toes in the offspring, so far as information could be obtained. This result is not in harmony with Poulton's observations, however, and may not be borne out by further information.

I have obtained several of these cats for breeding and future study.

Harry Beal Torrey.
Zoological Laboratory, Univ. of Cal.,
Berkeley, Cal., Sept. 6, 1902.

MAGNETIC WORK OF THE UNITED STATES COAST AND GEODETIC SURVEY PLANNED FOR JULY 1, 1902, TO JUNE 30, 1903.

(*a*) *Land Magnetic Survey Work.*—The determination of the three magnetic elements at four hundred to five hundred stations distributed principally in Virginia, New Jersey, Pennsylvania, Ohio, Michigan, Kansas, Nebraska, Texas, Arkansas and Florida.

(*b*) *Magnetic Observatory Work.*—The continuous operation of the four magnetic observ-

atories situated at Cheltenham (Maryland), Baldwin (Kansas), Sitka (Alaska) and near Honolulu (Hawaiian Islands). Also the selection of sites and preparations of plans for an observatory in Porto Rico or vicinity and another in the extreme western part of the United States.

(*c*) *Ocean Magnetic Survey Work.*—The inauguration of magnetic work on board ship in connection with regular trips of vessels engaged in coast survey work.

(*d*) *Special Investigations* conducted at the observatories and at educational institutions by persons available as 'associate magnetic observers.'

(*e*) *At the Office at Washington* a special effort will be made to bring all computations of field work performed and investigations conducted since July 1, 1899, up to date and to prepare results for publication. [The results for magnetic declination referred to January 1, 1902, embracing all observations up to June 30, 1902, are contained in the 'United States Magnetic Declination Tables for 1902,' now passing through the press. The results for magnetic dip and intensity up to June 30, 1902, are being prepared for publication and will appear in Report of the Superintendent of the Coast and Geodetic Survey for 1902.]

L. A. BAUER.

THE HUGH MILLER CENTENARY.

THE celebration of the centenary of Hugh Miller, the Scotch geologist and litterateur, took place in the picturesque little village of Cromarty, his native place, on August 22, and was the occasion of a large and enthusiastic gathering. Those present were very largely Scotsmen and the day was made one of special rejoicing in view of the extraordinary service rendered by Miller as a layman to the ecclesiastical disestablishment in Scotland, yet his services to geologic science and his unequalled achievement in clothing geologic facts in alluring literary garb were kept in the foreground. The ceremonies of the occasion began with an outdoor meeting at the foot of the fine shaft which bears at its summit a statue of Miller. This meeting was opened by the Provost of the town, Mr. Junor, and was presided over by Mr. Bignold, M.P. Addresses were delivered by Sir Archibald Geikie, former director of the Geological Survey of the United Kingdom; Dr. Rainy, principal of the Free Church College, Edinburgh; and Dr. J. M. Clarke, of Albany, who with Dr. C. R. Eastman represented the Geological Society of America. A luncheon followed in the largest hall the village afforded, though this was altogether insufficient to accommodate those who desired to attend, and while 250 sat down at table, as many more were turned away. At this function Sir Thomas Hanbury presided and speeches were made by Dr. John Horne, chief of the Geological Survey of Scotland; Rev. Dr. Muir, of the Glasgow Cathedral; Dr. Carnegie; Professor Middleton, of Oxford; Sir James Grant, president of the Royal Society of Canada, and others. The occasion was closed by an elaborate and elegant address by Sir Archibald Geikie on Miller's work and influence as a geologist. The effort which has been made by the people of Cromarty to raise a memorial to Miller in the form of a library and museum has not thus far been as successful as was anticipated, though the contribution from America has been substantial. It is believed, however, that this celebration which called forth widespread interest, great enthusiasm and strong editorials from all parts of Great Britain, will help to further the project which appeals to all who honor the memory or have felt the influence of this great man.

THE BRITISH ASSOCIATION.

THE Belfast meeting of the British Association is said by the British journals to have been one of the most interesting in its history. The programs were full, and there were a number of addresses and papers of special importance. The attendance was about 1,600 which was about 300 less than that of the preceding meetings at Bradford and Glasgow, and the meeting at Belfast in 1874 presided over by Tyndall. The meeting at Bristol in 1898, had an attendance of 2,446 and that of Liverpool in 1896 of 3,181. The attendance at the meetings of the British Association is

about equally divided between members and local associates, who subscribe for the meeting, and is consequently not very much larger than that of our own association. We publish above the first part of the address of the president, Professor Dewar, and hope to publish subsequently several of the addresses of the presidents of the sections and a report of the scientific proceedings. As already reported Sir Norman Lockyer was elected president, Dr. J. S. Garson, was made assistant general secretary in the room of the late Mr. G. Griffith, and Major P. A. McMahon, general secretary, in succession to Sir William Roberts-Austin. New members of the council are Sir W. Abney, Professor A. C. Haddon, Mr. C. Hawksley, Professor G. B. Howes, Professor W. W. Watts and Professor D. J. Cunningham. The meeting next year will be at Southport, and the following year at Cambridge. It is expected that the meeting in 1905 will be in South Africa.

Grants to committees for scientific purposes were made as follows:

MATHEMATICS AND PHYSICS.	
Rayleigh, Lord—Electrical Standards	£35
Judd, Professor J. W.—Seismological Observations	40
Shaw, Dr. W. N.—Investigation of the Upper Atmosphere	75
Preece, Sir W. H.—Magnetic Observations	40
CHEMISTRY.	
Divers, Professor E.—Study of Hydroaromatic Substances	20
Roscoe, Sir H. E.—Wave-length Tables of Spectra	5
GEOLOGY.	
Herdman, Professor—Fauna and Flora of British Trias	5
Marr, Mr. J. E.—Erratic Blocks	10
Scharff, Dr. R. E.—To Explore Irish Caves	40
Watts, Professor W. W.—Underground Waters of Northwest Yorkshire	40
Marr, Mr. J. E.—Life-zones in British Carboniferous Rocks	5
Geikie, Professor J.—Geological Photographs	10
ZOOLOGY.	
Herdman, Professor W. A.—Table at the Zoological Station at Naples	100
Woodward, Dr. H.—Index Animalium	100
GEOGRAPHY.	
Keltie, Dr. J. S.—Tidal Bore, Sea Waves and Beaches	15
Holdich, Sir T.—Scottish National Antarctic Expedition	50
ECONOMIC SCIENCE AND STATISTICS.	
Brabrook, Mr. E. W.—Economic Effect of Woman's Labor	25
MECHANICAL SCIENCE.	
Preece, Sir W. H.—Screw Gauges	5
Binnie, Sir A.—Resistance of Road Vehicles to Traction	90
ANTHROPOLOGY.	
Evans, Sir J.—Researches in Crete	100
Read, Mr. C. H.—Exploration of Stone Circles	5
Cleland, Professor J.—Anthropometric Investigation	5
Ridgeway, Professor—Anthropology of the Todas and Tribes of Southern India	50
Read, Mr. C. H.—Anthropological Photographs (balance in hand)	—
PHYSIOLOGY.	
Halliburton, Professor W. D.—The State of Solution of Proteids	20
BOTANY.	
Miall, Professor L. C.—Registration of Botanical Photographs	3
Farmer, Professor J. B.—Investigation of the Cyanophyceæ	25
Ward, Professor Marshall—Respiration of Plants	12
EDUCATIONAL SCIENCE.	
Sherrington, Professor—Conditions of Health Essential for School Instruction	10
CORRESPONDING SOCIETIES.	
Whitaker, Mr. W.—Preparing Report, etc.	20
	£960

SCIENTIFIC NOTES AND NEWS.

Dr. D. C. Gilman, president of the Carnegie Institution, has returned to the United States.

Dr. Andrew D. White, Ambassador to Germany, will present his letters of recall at about the middle of the month. His successor, Dr. Charlemagne Tower, is also interested in literary and scientific subjects, being a member of the American Philosophical Society and of the American Institute of Mining Engineers.

Dr. Henry C. McCook, known to scientific men for his publications on ants and spiders, has retired after a service of about thirty-three years from the pastorate of a Presbyterian church in Philadelphia owing to ill health.

Professors Josiah Royce and George H. Palmer, of the philosophical department of Harvard University, have leave of absence for the present year. Professor Palmer has sailed for England.

Brigadier-General William H. Forwood, U. S. A., who was recently retired as surgeon-general, was tendered a banquet on September 19 at Washington.

Professor Wilhelm Wundt, the eminent psychologist, has, on the occasion of his seventieth birthday, been made an honorary citizen of the city of Leipzig.

Professors Julius Wiesner and Karl Goebel, who hold respectively the chairs of botany at Vienna and Munich, have been elected corresponding members of the Göttingen Academy of Sciences.

Dr. Eduard Richter, professor of geography at Graz, has been made a member of the Vienna Academy of Sciences.

Dr. Adolf Engler, professor of botany at Berlin, is at present engaged in an expedition to Africa.

An expedition from the Liverpool School of Tropical Medicine under Major Ronald Ross, has gone to the Suez Canal to institute preventive measures against malaria.

Dr. Wilhelm Muthman, of the Munich Institute of Technology, has received a grant of 3,000 Marks for researches in inorganic chemistry, from the fund for German industry.

President Pritchett, of the Massachusetts Institute of Technology, has accepted the invitation of the trustees of the Lowell Textile School to deliver the address at the dedication of its new buildings, the date of which has not yet been announced.

Major Gorgas, chief of the sanitary department at Havana during the American occupation, has returned to the United States. Before leaving Cuba he was given a dinner by President Palmer, who expressed the gratitude of Cuba for the efficient services rendered the island and the city, especially in the suppression of yellow fever.

Dr. G. M. Guiteras, yellow fever expert of the Marine Hospital Service, has returned to the United States after an absence of three years in Cuba. He has been ordered to Philadelphia.

Professor C. R. Van Hise, who for a number of years has devoted himself particularly to the investigation of the metamorphic formations has been placed by the U. S. Geological Survey in charge of its studies of this important group. He is being assisted by Mr. C. K. Leith in the preparation of a comprehensive monograph of the Lake Superior region, by Dr. W. S. Bayley in the completion of field work in the famous Menominee district, by Dr. W. H. Hobbs in the continuation of surveys in Connecticut, where the metamorphic problems are of decided interest, and by Dr. Florence Bascom in areal and structural studies in the Pennsylvania district.

Professor Henry S. Williams, of Yale University, is devoting this season to the continuation of his studies, for the U. S. Geological Survey, of problems of the Devonian formations in Pennsylvania, New York and Maine, looking to a systematic correlation of the present knowledge of all the rocks of the country of Devonian age. He is being assisted by Mr. E. M. Kindle.

Professor Dr. Adolf Schmidt, of Gotha, has been appointed director of the Potsdam Magnetic Observatory in succession to the late lamented Professor Eschenhagen. He takes charge on October 1.

Mr. Edward R. Smart, in charge of trigonometric work in the Island of Trinidad, spent some time at the Coast and Geodetic Survey, familiarizing himself with the instruments and methods in use in the geodetic and magnetic work. Father Edmond Goetz, S.J., likewise familiarized himself at the Coast and Geodetic Survey with magnetic instruments and methods, preparatory to work he contemplates undertaking, starting out at Bulawayo, Rhodesia.

DR. L. A. BAUER left Washington on September 15 for a six weeks' inspection trip of magnetic work in the western states and to make the necessary preliminary observations for special magnetic investigations during the coming winter and spring in the northern part of Michigan in the vicinity of the Great Lakes.

IT is stated in *Nature* that letters received from Uganda give a good account of the progress of Mr. Budgett, Balfour traveling student of Cambridge, on his zoological mission to the Semliki. On July 13, he writes that he was proposing to start next day from Kampala for Lake Albert, where he would probably stay at Batyaba, near the Nile end, the *Polypterus* which he was in quest of being stated to be abundant at this spot. Afterwards his plans were to proceed southward to Fort Portal and thence to the Semliki valley, where he would make a general collection and look after the okapi in the neighboring forest. Mr. Jackson has most kindly allowed Mr. Budgett to have the assistance of one of his trained taxidermists.

IT has been decided to erect the statue of Pasteur by Falguière in the Avenue de Breteuil, Paris.

MR. WILLIAM NEALE LOCKINGTON died at Worthing in Sussex, England, on the 3d of August, at the age of about sixty years. Mr. Lockington was from 1878 to 1881 curator of fishes in the California Academy of Sciences. At this time he published a number of papers on the fishes and the crabs of the Pacific coast, the most important being a review of the flounders of California. He was the discoverer of a considerable number of interesting new forms. Before coming to California he had traveled somewhat widely in Spain and other parts of Europe and had achieved some reputation as a naturalist. After returning to England he was obliged by failing health to give up scientific work, but always retained a deep interest in natural history and in the affairs of California.—D. S. J.

DR. P. PLÓSZ, professor of physiology and pathological chemistry at Buda Pesth, has died at the age of fifty-seven.

Nature reports the death of Professor J. J. Hummel, principal of the dyeing department of the Yorkshire College, Leeds; and of Mr. Alexander Sutherland, registrar of the University of Melbourne, author of 'The Origin and Growth of the Moral Instinct.'

THE Academy of Science at Cracow has received from the state an appropriation of 61,000 crowns.

IT will be remembered that it was decided to close the meteorological observatory on Ben Nevis, owing to lack of funds, and the staff were told that their services would not be required after October. It has now, however, been decided to keep the Observatory open during the present winter, and it is hoped that the government will provide for it permanently.

THE board of health of San Francisco has issued a report reaffirming the existence of the plague in San Francisco. The mayor of the city dismissed the board last March, owing to its making a truthful report in regard to the plague, but the action of the mayor was not upheld by the courts.

THE International Congress on Tuberculosis meets at Berlin from October 22–26. The subjects suggested for special discussion are: (1) position of Governments with regard to the prevention of consumption; (2) obligation to give information to the police; (3) organization of dispensaries; (4) the task of schools with regard to the prevention of consumption; (5) precautions against the dangers of milk; (6) tuberculosis during infancy; (7) protection of labor and prevention of consumption; (8) classification and different modes of accommodating consumptives.

THE society for the protection of the interests of the German chemical industry, recently in session at Frankfort, has unanimously passed a resolution against the prohibition of the use of boric acid for the preservation of meats, and has appealed to the Bundesrath to reverse its decision.

DR. LOUIS ELKIND writes to the London *Times*: "It is rather curious that, though Professor Virchow's name has been well known throughout the civilized world for a long period, very few people know how to pro-

nounce it, Germans themselves being almost as mistaken in their pronunciation as foreigners. Never was this general error as plainly emphasized as at the celebrations held in honor of the great scientist's 80th birthday. The delegates whom he received on that occasion had each his own way of pronouncing V-i-r-c-h-o-w, Lord Lister, for instance, speaking as if the word were spelled Wirtschau, Signor Baccelli, Wirkoff, while his French and Russian colleagues pronounced his name in such a way that it was by no means easy to understand whom they meant—Wirschoff, Wirhoff, and Wirchoff respectively. At the banquet which was given by Count von Bülow in the late Professor Virchow's honor, and which practically brought the festivities to a close, Professor Harnack addressed the guest of the evening as Herr F-i-r-ch-o—that is to say, the F is accentuated as softly as possible, as in the English 'fair' and the Russian 'Feodor,' while the 'w' is dispensed with. This greatly delighted the veteran pathologist, and he remarked that never before had he heard his name pronounced properly. Subsequently, he dwelt upon the origin of his name, saying that he had been able to trace it to a small village and a lake in Pomerania, both of which are named Virchow, which word the natives pronounce exactly as Professor Harnack had done. It may be of interest if I add that a leading German philologist devoted considerable time to the subject of the origin of Virchow's name, and came to the conclusion that it was Slavonic. The Slavs, he thought, who bore it, were settled in Pomerania about the fifth century of the Christian era, and gave one of their names to the village and the lake."

UNIVERSITY AND EDUCATIONAL NEWS.

It is announced that the bequest to the Princeton Theological Seminary made by Miss Mary Winthrop, of New York, will amount to $1,400,000.

At Harvard University students can hereafter complete the requirements for the A.B. degree in three years without other requirements than that the necessary number of courses should have been taken. Hitherto students who received honors could do this, others being required to wait a year before the degree was conferred.

The Hon. John D. Long, formerly secretary of the navy, has been elected president of the board of overseers of Harvard University.

Superintendent Edwin G. Cooley, of the Chicago public schools, has been offered the presidency of the University of the State of Washington.

Professor Thomas F. Holgate, head of the department of mathematics of Northwestern University, has been elected dean of the College of Liberal Arts.

Dr. John Marshall, professor of chemistry and toxicology and dean of the medical department of the University of Pennsylvania, has declined reelection to the office of dean, a position he has held for eight years, in order to devote himself more exclusively to scientific work. Dr. Charles Frazier has been appointed dean of the department.

Dr. William B. Savery, of Fairmount College, Kansas, has been elected to fill the chair of philosophy at the Washington State University, left vacant by the resignation of Dr. F. W. Colegrove.

Dr. Frank S. Wrinch, Ph.D. (Leipzig), of Toronto, has been appointed demonstrator in experimental psychology in Princeton University.

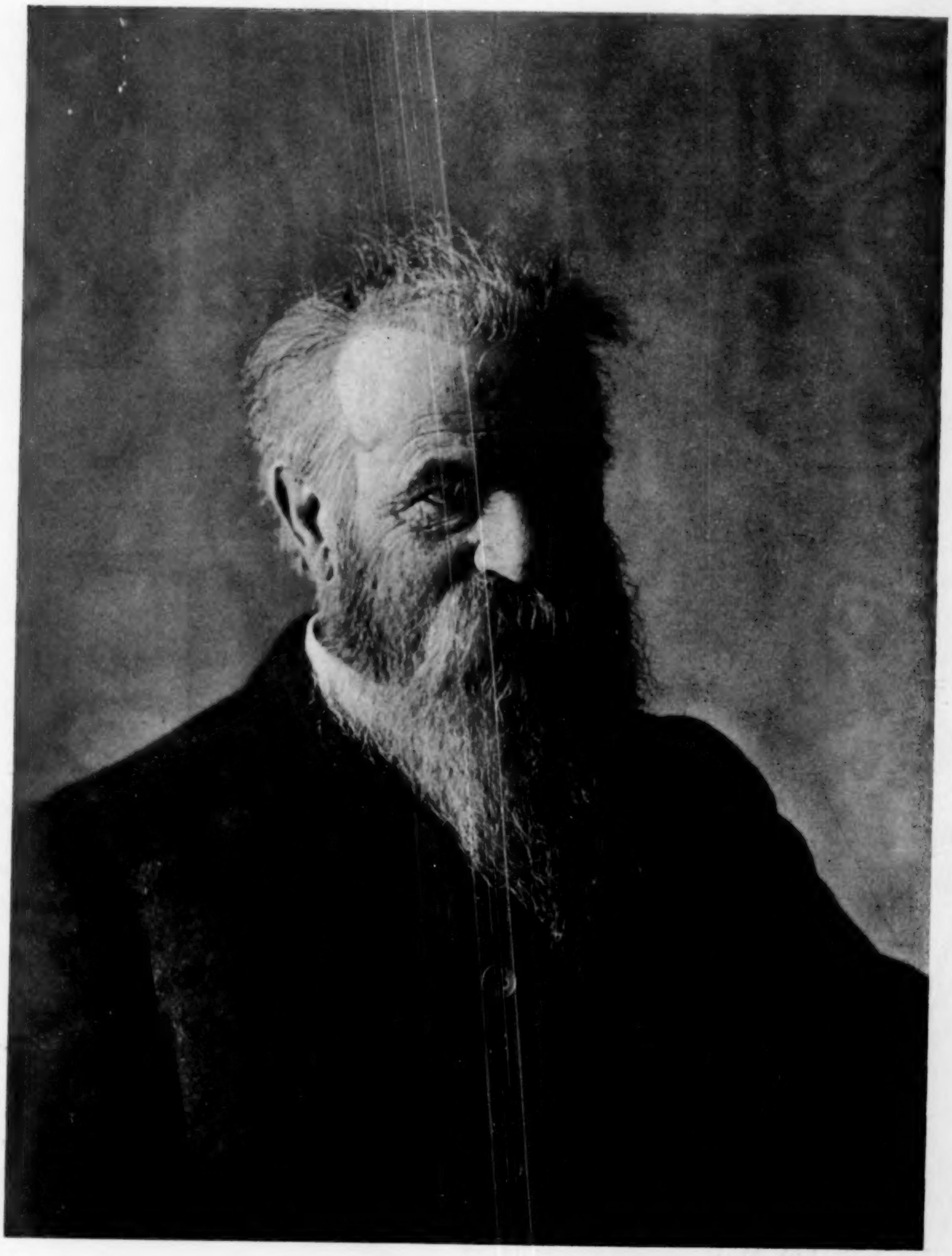
Dr. J. W. L. Jones, Ph.D. (Princeton), has been appointed professor of philosophy and education in Heidelberg University, Ohio.

Dr. Wilhelm Windelband, professor of philosophy at Strassburg, has received a call to Heidelberg.

Dr. Heinrich Maier, associate professor of philosophy at Zurich, has been called to Tübingen as successor to Professor E. von Pfleiderer.

Dr. Wilhelm Trabert has been appointed to a full professorship of cosmical physics at University of Innsbruck.

Dr. Oscar Zoth, professor of physiology at Graz, has been called to Innsbruck as successor to Professor M. von Vintschgau.



J. W. Powell